

DIVERGENCES IN THE HEBREW OF THE SCROLLS: A REVIEW OF ELISHA QIMRON'S *A GRAMMAR OF THE HEBREW OF THE DEAD SEA SCROLLS*

Abstract

The new grammar of the Hebrew of the Dead Sea Scrolls by Elisha Qimron presents a synthesis of this scholar's numerous illuminating insights. Unfortunately, too many errors (e.g., inaccurate references, misreadings of Hebrew words, and imprecise language) distract from and diminish the analysis. In addition, certain linguistic features (e.g., dialect and register, misspellings due to graphic confusion) seem to be deemphasized or neglected in the study. The result is that scholars should use this resource with caution.

Introduction

IN 2018 Elisha Qimron's *A Grammar of the Hebrew of the Dead Sea Scrolls* was published by Yad Yizhak Ben Zvi. In general, it has much to recommend it, but unfortunately it contains numerous problems, ranging from inaccurate cross-references and misreadings of Hebrew words to more global methodological inadequacies (like the neglect of an explicit consideration of register and dialect). First, this review will present the general structure of the grammar and summarize some of its positive points, before moving on to address general methodological concerns and then more specific errors.

The grammar represents an expansion of the author's dissertation (p. 15), which was completed in 1976, and is a good deal more thorough than the 1986 version published in English. (1) The grammar's

(1) Elisha Qimron, *Grammar of the Hebrew Language of the Scrolls of the Judean Desert* (Jerusalem: Hebrew University, 1976) (in Heb.); idem, *The Hebrew of the Dead Sea Scrolls*, HSS 29 (Atlanta: Scholars Press, 1986).

observations are supported by numerous citations of the scrolls based on the author's close examination of the manuscripts (which led to his three volume edition of the scrolls). (2) Here finally one will find the author's many erudite observations on the scrolls and their grammar, presented succinctly and clearly in one publication. The synthesis of so many new and accurate readings combined with the thorough treatment of the grammar (often with comparisons to earlier and later dialects and varieties of Hebrew as well as to other languages) will make this the go-to resource for anyone interested in learning about the Hebrew language of the scrolls. In short, if one is interested in reading the scrolls in Hebrew, one must consult this resource.

The book begins with summaries of the relationships between the Hebrew of the scrolls and that of the Bible and later Rabbinic works. It assesses briefly the similarities and differences to Aramaic, before going on to isolate various other general topics, as expressed in the titles of the sections, like: "Unique Features," "Uniformity of DSS Hebrew," "Orthographical and Phonological Variants." There follows a brief summary of past studies of the Hebrew of the scrolls and an outline of the author's perspective and set of assumptions. In essence Qimron approaches the language of the scrolls without assuming that it should conform to or align with another type of Hebrew (e.g., Samaritan, Tiberian, Babylonian, that of the Greek or Latin transliterations). This is entirely appropriate since it is beyond doubt that the Hebrew of the scrolls evidences features not shared by these other types of Hebrew (see p. 41 for some examples). It is also a reminder that our familiarity with the Tiberian vocalization tradition should not prejudice us against finding unique traits in the Hebrew of the scrolls and/or links with other traditions of Hebrew.

The grammar proceeds with separate major sections on orthography, phonology, verbal morphology, personal pronouns, nominal morphology, the "Interchange of Weak Forms," particles, and syntax. The book concludes with two indices, one including a selection of the various Hebrew words referred to in the book and the second including a selection of passages cited. Neither is comprehensive. There is no glossary of words like that found in Qimron's earlier grammar, *The Hebrew of the Dead Sea Scrolls*.

Among the various observations on orthography, it is interesting to note that the *yodh* appears to represent a penultimate *e* vowel in infinitives and nouns with suffixes, as in לעִבְדִּיךָ "to worship you" 1QH^a IV, 26 (p. 68, §A 3.2.1) and בּוֹרְאִיכָה "your creator" 1QIsa^a at

(2) Elisha Qimron, *The Dead Sea Scrolls: The Hebrew Writings*, Between Bible and Mishnah, 3 vols. (Jerusalem: Yad Yizhak Ben Zvi, 2010–2014).

Isa 43:1 (p. 69, §A 3.2.1). Thus, singular forms with the 2ms pronominal suffix are sometimes graphically indistinguishable from the plural noun with the same suffix. The same ambiguity pertains to nouns with the 3ms pronominal suffix (p. 73, §A 3.3). It is easy to see how such observations can help refine our understanding of the texts. Take, for instance, the phrase **המה כור הוריכה** appearing in 4Q416 2 iii, 17 (also reflected in 4Q418 9+9a-c, 18). Since its publication, scholars have pondered the final word, whether it should be construed as singular or plural. (3) Given the numerous other examples of singular forms with the final **-יכה** pronominal suffix, it seems now easier to suggest **הוריכה** is singular with “furnace” as grammatical subject: “they are the womb (lit., furnace) that gave birth to you.”

In the section on phonology (p. 103, §B 1.2.1.3), it is significant that the sequence *-ūhū* sometimes contracted to *-ū* (a long *u* vowel) or *-û* (an ultra-long *u* vowel). Thus, some forms with a simple mater *waw* may be intended to convey a 3ms pronominal object suffix (i.e., **בקשו**, “they sought” 1QS V, 11 may really reflect **בקשוהו** “they sought him”). Similar observations on the grammar are easily multiplied.

The strength of the grammar lies, in part, in its presentation of the Hebrew of the scrolls as a single spoken dialect. Qimron believes that the scrolls in large measure derive from “a group of people who left Jerusalem” and that their language reflects “the language of the Capital in the late Second Temple period” (p. 33). He goes on to state unequivocally that the Hebrew of the scrolls “records the spoken language of that time, and has not been distorted by later scribes who did not speak this language” (p. 33). Believing this allows the author to find coherency in sometimes contradictory and conflicting data. This, in turn, results in a grammar that is relatively clear and forthright about many different phenomena.

Another fundamental strength of Qimron’s approach is that it assumes many idiosyncratic spellings do not reflect graphic mistakes, but instead reflect the language of the scribes. Thus, to take the example cited just above, a spelling like **בוראיכה** “your creator” (1QIsa^a at Isa 43:1, p. 69, §A 3.2.1) is not dismissed as a graphic mistake for **בוראכה**. Instead,

(3) E.g., John Strugnell and Daniel J. Harrington (*Qumran Cave 4.XXIV: Sapiential Texts, Part 2: 4QInstruction (Mūsār Lē Mēvīn): 4Q415 ff.*, DJD 34 [Oxford: Clarendon, 1999], 121) suggest it could formally be singular, but should be plural given the presence of **המה** “they” (i.e., mother and father). Matthew Goff (*4QInstruction, Wisdom Literature from the Ancient World 2* [Atlanta: SBL, 2013], 111) also suggests that the form is a masculine plural participle from **הרה**. Jean-Sébastien Rey (*4QInstruction: sagesse et eschatologie*, STDJ 81 [Leiden: Brill, 2009], 189) offers different possible interpretations, but suggests it should be plural if it is a *qal* participle from **הרה**. Eric D. Reymond (“The Poetry of 4Q416 2 iii 15–19,” *DSD* 13 [2006]: 177–93 [spec. 181 n. 17]), however, views it as singular.

the spelling with *yodh* is taken as expressive of a vowel that came between the stem of the noun and the pronominal suffix. In a similar way, the initial spelling of **יָא** (without *yodh*) in 1QIsa^a at Isa 13:22 is interpreted (p. 95 n. 133, §A 9) as evidence that the *yodh* in such words was not pronounced. Qimron writes: “This last spelling [i.e., **יָא**] . . . makes sense only if this word was not pronounced with consonantal *yod*” (ibid.). This approach is beneficial in the sense that it allows the author to utilize a maximum amount of data to inform his analysis of the grammar and provides a wealth of information that might be ignored if one were to dismiss such spellings as graphic mistakes.

Despite these benefits, the grammar’s method also seems problematic due to some of these same underlying assumptions. In what follows I address first my general difficulties with the grammar, specifically: 1) the absence of reference to registers and dialects; 2) the tendency to underestimate linguistic variation; 3) the tendency to draw conclusions from too little data; and 4) the tendency to deemphasize graphic errors in the spelling of words. Following these general remarks, I list specific observations according to page and paragraph number.

1.

As noted above, Qimron approaches the scrolls as primarily deriving from Jerusalem in the late Second Temple era, so he does not explicitly recognize in them different regional dialects. Nevertheless, he does recognize that certain features may not have been shared among all speakers. For example, in suggesting evidence for the coalescence of the phonemes *š* and *s*, he writes “it was perhaps unique to a particular segment of the speech community” (p. 121, §B 6.3). In another case, noting the 3fs suffix *qatal* (i.e. perfect) form **היית** “she was” in 1QIsa^a at Isa 17:1 and 19:17 (and perhaps **היית** in 11Q4 at Ezek 5:15), he writes that the form “can be considered dialectal.” Unfortunately, he does not elaborate on the sub-dialects of the speakers and what the implications of this might be. In my view, because the scrolls themselves seem to refer to multiple groups of sectarians in different locations and this seems confirmed by descriptions by Philo and Josephus, (4) we should assume to find in them some evidence of different regional dialects of Hebrew. Nevertheless, it must be admitted that such identifications are particularly difficult to pinpoint.

(4) See the summary and references in John J. Collins, “Sectarian Communities in the Dead Sea Scrolls,” in *The Oxford Handbook of the Dead Sea Scrolls*, ed. Timothy H. Lim and John J. Collins (Oxford: Oxford University Press, 2010), 151–72.

In addition, the grammar does not consistently distinguish between different registers (e.g., a colloquial versus a literary register), (5) though the existence of such registers seems to be implied through the occasional reference to “vulgarisms.” For example, some *qal* verbs exhibit an *a* theme vowel in the Tiberian vocalization tradition, but are rarely represented with a *waw* in the DSS, implying an *o* or *u* theme vowel. Qimron suggests (p. 188, §C 3.2.1) that the correction dots around the *waw* in יִהְרֹב (implying יִהְרֹב “it will dry out” (1QIsa^a at Isa 19:5) and in יִשְׁחַק (implying יִשְׁחַק “he will laugh” (1QpHab IV, 6) may demonstrate that the pronunciations with *o* or *u* “were still regarded as a vulgarism and perhaps prevailed in the spoken Hebrew of the scribes.” In a similar way, the predominance of אִשֶּׁר in the DSS is characterized as possibly being due to the fact that the writers viewed the alternative “-ש as a vulgarism” (p. 425, §H 4.7.1).

Since the grammar, as explained above, is focused on identifying those linguistic features that are part of the “spoken language of that time” (p. 33), it is unclear how one should understand these features of the language. The term “vulgarism” itself seems to imply that such features should not be considered part of “DSS Hebrew,” even though the scribes might have used them in their spoken language. Referring to registers would eliminate this confusion. In the above examples, the *a* theme vowel in certain *qal* verbs and the use of אִשֶּׁר can be construed as part of the literary register of DSS Hebrew, while the so-called vulgarisms can be considered as part of the colloquial register. Both should be considered DSS Hebrew since both are attested in the scrolls. Moreover, neither the colloquial nor literary registers are identical to the Hebrew reflected in the MT consonantal text or in the Tiberian vocalization.

The many ways that the colloquial register might have differed from the Hebrew of the MT and Tiberian tradition are easy to infer based on the many grammatical differences observed in the scrolls. It is important to notice as well that the literary register of DSS Hebrew is distinct from the Hebrew of the MT consonantal text and Tiberian vocalization tradition; and, this is documented in the grammar too (though it is not explicitly described in this way). For instance, the short-*yiqtol* (or apocopated) verb forms (e.g., of weak roots and in the *hiphil*) sometimes appear after *waw*, even in cases where the verb form is not part of a *wayyiqtol* or volitive construction and where, in fact, it expresses the indicative mood of the future (see pp. 160–69, §C 2.1.3 and comments below). For example, וַיֵּכֶן in 1QS XI, 13 looks (out of

(5) Though, it should be mentioned, the number of registers need not be limited to just these two.

context) like it should express the past indicative “and you established” (or a volitive or purpose/result sense: “and may he establish” or “so that he may establish”) but the context instead clearly favors the future indicative *וּמִשְׁחַת יִחַלֵּץ נַפְשִׁי וַיְכַן לְדֶרֶךְ פַּעֲמֵי* “he will withdraw my soul from the pit and steady my steps on the path.” Similarly, *וַיִּקְטַר* in 11Q19 XXIII, 16 would (out of its context) seem to express the past “and he burned,” but the context clearly implies “and he will burn.” This peculiar use of the *w*+short-*yiqtol* seems unlikely to have been part of the colloquial spoken register, where the future indicative would have been communicated, presumably, by the regular (long-)*yiqtol* conjugation. Rather, the peculiar syntagm in the DSS likely developed in the literary register derived from the use of this same syntagm to express a volitive mood or purpose/result sense (see, e.g., Joüon-Muraoka, §116). (6)

In the end, distinguishing between colloquial and literary registers is important since ignoring such dimensions sometimes contributes to misleading statements. For example, in the description of the paragogic *nun*, Qimron identifies several cases of this morpheme among the non-biblical scrolls, but only where it appears in phrases that quote or allude to biblical texts; in addition, he finds numerous cases in the biblical scrolls. His conclusion is that the morpheme was “practically unused” in DSS Hebrew (p. 157, §C 2.1.2.1). What, I assume, he means is that it was unused in the colloquial register.

There are many examples of the paragogic *nun* that are not listed in the grammar: Accordance lists 27 instances of the morpheme among the non-biblical scrolls and 135 instances among the biblical scrolls. Many of these, to be fair, are on words that are partially reconstructed. Nevertheless, even given this qualification, the morpheme still appears fairly frequently. It occurs among the biblical scrolls in places where the MT has no paragogic *nun* and, among the non-biblical scrolls, it occurs in non-biblical contexts (e.g., *יִדְעוּן* and *יָשׁוּבוּן* [see comments below to pp. 157–59, §C 2.1.2.1]). All this suggests that the morpheme was, in fact, used, but it was part of the literary register, not the colloquial. The characterization of it as “unused” reflects the grammar’s interest in the underlying colloquial language.

In addition to dialect and register, one can sometimes glimpse more specific linguistic profiles. The grammar does not directly explore such nuances, though it does sometimes refer to exceptional phenomena not shared among all speakers (e.g., the coalescence of the phonemes *š*

(6) Note that in classical Biblical Hebrew of the MT the *waw* conjunction and the (short-)*yiqtol* form are sometimes separated from each other where the sense seems to be that of purpose/result (see Joüon-Muraoka, §116.i).

and *s* and the 3fs suffix *qoṭal* with final *-t* [e.g., הִיִּית “she was”], mentioned above). Nevertheless, there is more evidence that the scribes who copied some scrolls had specific linguistic peculiarities not shared by other scribes. In a recent article in *DSD*, I studied three linguistic features found prominently in 1QS and 1QIsa^a: the Aramaic *־הי* suffix, *heh* in the prefix of *hiphil yiqtol* verbs, and the exchange of gutturals (where the etymological guttural is replaced by another guttural). (7) These features appear both in 1QS and 1QIsa^a though these manuscripts were copied by different scribes; conversely, these same features are found rarely or not at all in the other manuscripts copied by the scribe of 1QS (i.e., in 1QSa, 1QSB, 4Q53 [4QSam^b], and 4Q175). (8) In the end, the distribution of these features would seem to reflect an individual (or individuals) whose Hebrew was qualitatively different from that of his (or their) contemporaries or near contemporaries.

The fact that most DSS do not exhibit these features reflects the obvious fact that some scribes knew the traditional manner of spelling words. Since reading and writing often involve an oral component, even if that oral component is exclusively in the mind (Goethe’s *innere Aufführung*), it seems likely that some scribes would be conversant in some variety of the literary register of the language, as reflected in the so-called conservative spelling. (9) In the case of the gutturals, for example, I would imagine that a few scribes likely could not distinguish the gutturals at all, while others could only distinguish the gutturals when concentrating, and still others could distinguish them consistently (at least in their reading/writing register).

(7) Eric D. Reymond, “The Scribe of 1QS, 1QSa, 1QSB, 4Q53 (4QSam^c), 4Q175, and Three Features of Orthography and Phonology,” *DSD* 25 (2018): 238–54.

(8) This is contrary to what we might expect. Since these features, especially the exchange of gutturals, would be a relatively unconscious tendency, one would assume that this feature at least would be spread more or less evenly throughout the manuscripts of the 1QS scribe. Since they are not and, in fact, appear prominently only in another manuscript copied by different scribes (i.e., 1QIsa^a), it stands to reason that the presence of these features does not directly reflect the language of the 1QS scribe, but rather likely reflects that of another scribe / author who had a hand in copying and / or writing the *Vorlage* of 1QS and who also helped copy 1QIsa^a or one of its *Vorlagen*. The scribe of 1QS and perhaps those of 1QIsa^a too were simply transmitting their texts as they found them, the conscientiousness of the 1QS scribe being reflected in the various gaps and interlinear insertions left in some places of 1QS, column VII. This conscientiousness, obviously, does not eliminate the possibility that some elements of 1QS and the other scrolls he copied are the result of this scribe’s inattention or confusion. Alternatively, perhaps the concentration of these orthographic / morphological deviations are attributable to a common reciter, who recited both of these texts for the scribes of 1QS and 1QIsa^a.

(9) See A. K. Gavrilov, “Techniques of Reading in Classical Antiquity,” *Classical Quarterly* 47 (1997): 56–75 (spec. 69).

Such a heterogeneous community of readers and writers makes better sense of the linguistic evidence preserved in the orthography of the Hebrew scrolls. It also better accounts for the textual fluidity and diversity seen in the scrolls, not to mention the social context implied by the scrolls themselves and contemporary accounts, both of which suggest multiple groups of sectarians who were not isolated in Jerusalem. Since, as George Brooke and Mladen Popović have emphasized, reading, writing, and interpreting texts were at the core of the community's identity, scribes would have likely been from diverse backgrounds (geographically, linguistically, socially, intellectually).

2.

The second general problem I have with the grammar's approach relates to where one should expect linguistic variation and where one should expect consistency. Qimron recognizes how the scrolls evidence numerous types of variations in particles, verb stems (i.e., *binyanim*), verb types (e.g., infinitive vs. *yiqtol*), particles, and lexemes (see pp. 42–44). He also calls attention to the fact that individual scribes spelled words in different ways, noting, for example, on p. 41 that the realization of specific features of the language depends on “the preferences of each scribe.” (10) Yet, he typically views spellings with and without maters as reflecting the same basic pronunciation. He writes: “In this grammar, I contend that the difference between long and short forms is merely orthographic” (p. 47).

His reasoning is, in part, reflected in his observation (p. 46) that even in scrolls (like 1Q8 = 1QIsa^b) that exhibit “conservative” spelling one finds traits that align with the Hebrew of scrolls that exhibit less conservative spelling. He writes (p. 47) that the single example of הוּא “he” in 11Q19 (at LIV, 11) confirms that the 3ms independent pronoun was articulated universally in this scroll with final *-ā*, though in its other thirty-four occurrences it is spelled הוּא. He concludes that words spelled without maters in scrolls like 1Q8 and 11Q19 are simply orthographic variants of the forms with maters. This presumes a single oral (and mental?) realization of forms like כִּם- and כִּמָּה- (both realized as *-kemma*). (11) Similarly the alternative 3mp pronominal suffixes ׁם(ה)- and מָה(ה)- are both understood to have a single oral realization (i.e., *-hemma*). (12)

(10) Note as well that on p. 45 he recognizes that “many individual manuscripts have peculiarities of their own.”

(11) See p. 47. He also writes: “The prevailing form [i.e., כִּם-], probably a defective spelling” (p. 280, §D 2.5).

(12) He writes concerning the occurrence of these alternative forms “side by side” in the same text as “probably orthographic variants” (p. 283, §D 2.6.2). The same

The question presents itself: Why should we assume consistency in the pronunciation of these morphemes, pronouns, and other words when their orthography is inconsistent? Such insistence is especially confusing given the fact that Qimron has noted so many cases of linguistic variation which are not associated with defective spellings (e.g., ללכת “to go” in 1QS IX, 9 and לההלך “to go” in 4Q258 VII, 9).

Although it is not stated, one gets the sense that the regularity of such pronunciations is considered a fundamental quality of a spoken language or dialect. Qimron makes frequent reference to the Samaritan oral tradition of Biblical Hebrew for explanations of forms he finds in the scrolls, especially the pronunciation of pronouns. He assumes that DSS Hebrew reflects a separate but coherent tradition of Hebrew just as Samaritan Hebrew reflects its own separate but coherent tradition. It is doubtless true that certain features of certain scrolls are similar to features of Samaritan Hebrew (e.g., weakening of gutturals, the splitting of so-called ultra-long vowels, pronominal suffixes ending in *-a* or *-ā* etc.). Nevertheless, analogy to the Samaritan tradition is not always convincing. Simply put, the scrolls may not reflect the kind of coherency and consistency found in the Samaritan tradition of Biblical Hebrew.

What we know of the Samaritan tradition is mostly based on information from the twentieth century CE. (13) It seems likely that certain aspects of the tradition developed over time. (14) Even if the oral tradition reflects an ancient pronunciation, it continued to be spoken “even after the 2nd century C.E.” (15) Thus, its form today does not

inference is made for the independent forms, but the correspondences are articulated in a less hesitant way (i.e., there is no “probably”): “הם = המה” (p. 58, §A 1) and “Evidently one should take הם as defective spelling of *hemma*” (p. 263, §D 1.6). Nevertheless, he does note that the 3mp suffix on 1cp and 3cp *qṭal* conjugation forms (after the long *ū* and *ī* vowels) likely does not have a final *-a* (p. 282, §D 2.6.1) and that the pronunciation of the suffix on forms like שמרם and דברם is less clear” (idem).

(13) See, e.g., Ze’ev Ben-Hayyim, *The literary and oral tradition of Hebrew and Aramaic amongst the Samaritans*, 5 vols. (Jerusalem: The Academy of the Hebrew Language, 1957–1977), in Hebrew; idem, *A Grammar of Samaritan Hebrew: Based on the Recitation of the Law in Comparison with the Tiberian and Other Jewish Traditions* (Jerusalem: Magnes; Winona Lake, IN: Eisenbrauns, 2000).

(14) Note, e.g., what Ben-Hayyim (*A Grammar of Samaritan Hebrew*, 227) writes in relation to the 3mp independent pronoun *imma* “they”: “We learn from the 13th century grammarian Abū Sa’īd that in the past, an *a* vowel served in place of the *i* used today before geminated מ or נ throughout the system of pronouns . . .”

(15) Moshe Florentin, “Samaritan Hebrew: Biblical,” in *Encyclopedia of Hebrew Language and Linguistics*, 4 vols. (Leiden: Brill, 2013), 3:445–52 (spec. 3:445).

necessarily reflect the idiom of the Second Temple era. Moreover, it seems likely that just as the Samaritan Pentateuch evidences a certain degree of harmonization (e.g., the exclusive use of הֵם “they” rather than variation between הֵם and הִמָּה), so the Samaritan vocalization tradition has also experienced some degree of harmonization (e.g., exclusive articulation of הֵם as *imma* “they”) and that the lack of variation in this dialect reflects not simply a spoken variety of ancient Hebrew, but also a self-conscious attempt at creating coherency (and thus suggesting legitimacy) in the oral tradition.

Certainly, there are sometimes clear cases where assuming such consistency in the DSS makes sense. For example, it seems likely that many (most?) scribes articulated the 3ms pronominal suffix on singular and plural nouns -ō, even when the pronoun is written יִי- (which corresponds to -aw on plural nouns in the Tiberian tradition). This is inferred not only from the spelling of plural words with יִי- (e.g., לִפְנֵי “before him” 4Q266 2 ii, 2) as well as the spelling of singular words with יִי- (primarily in 1QIsa^a, e.g., יָדִי “his hand” at Isa 5:25 ≈ MT יָדוֹ, but also in non-biblical texts like בְּנִפְיִי “by himself” 4Q158 7–8, 10 ≈ בְּנִפְיִי in Exod 21:3 [see pp. 269–70, §D 2.3.2.1]), but also counter examples spelled unusually to indicate a consonantal *waw* (עִשָּׂאוֹ ‘*ēsāw* “Esau” 4Q223–224 2 ii, 4; or the *aleph* indicates a glide, as Qimron suggests, ‘*ēsā*”u [see p. 131, §B 8.4.4]), as well as the assumption of penultimate stress (together with the expectation of diphthong contraction for unstressed historical -aw). (16)

Yet, we need not presuppose a common pronunciation for all pronouns or other forms that alternate between apparently defective and plene spellings. Just as the consonantal text of the MT as well as the Tiberian vocalization tradition imply two forms of the 3mp independent pronoun (הֵם and הִמָּה), so the scrolls seem to suggest two forms (הֵם and הִמָּה). To further demonstrate such variation it is useful to consider examples not involving the presence or absence of maters and which occur in close proximity to each other (proximity being one of the indications of common pronunciation according to Qimron). (17)

One can find only a limited number of examples of morphological and/or pronominal variation within these parameters. Nevertheless, there are some. Note, in the MT, the two forms of the preposition לְ “to” with the 3mp pronominal suffix, which occur in juxtaposed verses:

(16) See Eric D. Raymond, *Qumran Hebrew: An Overview of Orthography, Phonology, and Morphology*, RBS 76 (Atlanta: SBL, 2014), 144–47.

(17) E.g., he writes “In fact, doublets such as הֵם/הִמָּה . . . occur side by side even in one single passage without any reason. This indicates that the difference in such cases is orthographical rather than morphological” (p. 45). See also p. 283, §D 2.6.2 and the further references in footnote 12 above.

לָמוֹ “to them” (Ps 78:24) and לָהֶם “to them” (Ps 78:25); and in the same chapter: לָמוֹ “to them” (Lam 4:10) and לָהֶם “to them” (Lam 4:4). (18) Such variations also occur in the scrolls. The same two prepositional phrases occur in identical expressions: בְּצָר לָמוֹ “in their distress” (1QpHab V, 6; 4Q178 1, 2) and בְּצָר לָהֶם “in their distress” (4Q387 2 ii, 2). Furthermore, one finds in close proximity an alternation between לָמוֹ “to them” (1QS IX, 22) and לָהֶם “to them” (1QS IX, 19); and לָמוֹ “against them” (1QH^a XII, 27) and לָהֶם “them” (1QH^a XII, 19). Among the biblical scrolls, note also the variation in relative pronouns within one verse: אֲשֶׁר־יְהוָה אֱלֹהֵינוּ שְׂכָנָה לּוֹ אֲשֶׁר־יְהוָה אֱלֹהֵינוּ “blessed is the people whose (life) is like this, blessed is the people whose God is the Lord” (11Q5 at Ps 144:15; cf. the MT which has שׁ in both instances). (19) In none of these examples can the variation be construed as representing a common, underlying pronunciation.

If such forms can alternate with one another, then it seems possible that הֶם *hēm* (or for some scribes *’ēm*) could alternate with הֵמָּה *hēm̄mā* (or *’ēm̄mā*) even if any individual reader could choose to ignore the specific vocalization implied by the spelling of a given word. The variation may not have been a feature of the colloquial register, but perhaps only the literary register, or perhaps it reflects competing dialects, etc. (20)

3.

Although I think it is important to take seriously the possibility that rare irregular spellings may be reflective of some phonetic reality masked by otherwise conservative and relatively “defective” orthography, this cannot be the only explanation. (21) Too often, it seems, a minority of plene forms determine the understanding of the more numerous “defective” forms. This is demonstrated on p. 56, §A 1 where, in introducing the orthography of the scrolls, Qimron offers observations

(18) Similarly, two forms of עִם “with” occur with pronominal suffix in close proximity: עִמָּךְ and עִמְּךָ in Gen 21:23; עִמִּי and עִמְּדִי in Job 10:17.

(19) Similarly, note: וְאֵין שִׁיעֶשֶׂה יְהוָה . . . אֲשֶׁר חָפֵץ יְהוָה “what the Lord desires . . . there is no one who can do . . .” (11Q5 at Ps 135:6).

(20) Such variation is, of course, common. In English, formal literary writing requires the use of uncontracted forms like “cannot,” though when presenting a paper at a conference, it would not be surprising for a speaker to read what is written “cannot” as “can’t.”

(21) Note that spellings can only be considered defective if one considers that they were pronounced with vowels not reflected in the orthography. Since I consider this questionable in a number of instances, I do not consider such forms defective.

on the different spellings of the 2fs *qatal* conjugation in the MT (i.e., קָטַלְתָּ vs. קָטַלְתָּי and קָטַלְתָּי). He asserts that:

the evidence of the original final *i* is supported by the Biblical consonantal text, by Samaritan Hebrew, and by the Dead Sea Scrolls . . . In fact, it is the existence of the form *-t* (rather than *-ti* or *-ta*) that requires proof (p. 56, §A 1).

Is it really true that the Biblical consonantal text supports the existence of final *i* rather than final *-t*? The forms that orthographically and/or phonetically are represented with a final *i* are found almost exclusively in texts from the Exile or later (i.e., in Jeremiah, Ezekiel, Micah 4:13, and Ruth; see GKC §44h). Qimron notes (p. 155 n. 8) that one such form is also to be found in Gen 27:13 (sic, really 27:12), וְהִבְאֵתִי, though the context clearly allows for the 1cs, which is supported by the LXX (1cs), Targ. Onk. (1cs), Targ. Neof. (1cs), Targ. Ps-Jon. (1cs) (though not by the Peshitta or Samaritan Pentateuch).

The time period of the Exile and Second Temple period, of course, is the same time period when we see an increasing rise in Aramaisms in the biblical text. Given the relatively few forms that imply a final *i* and their concentration in later texts, why would these alternative forms not be considered a feature of later Hebrew, perhaps influenced by Aramaic or perhaps reflecting the influence of another dialect of Hebrew or even the idiolects of individual writers or scribes? (22)

In addition, Qimron compares this two-fold orthography for the 2fs *qatal* conjugation (both presuming a final *i* according to him) with the defective and plene spelling of the 2ms *qatal* form (קָטַלְתָּ and קָטַלְתָּה). This seems like a misleading analogy given the distribution of the plene spellings of the 2ms *qatal*. In contrast to the 2fs קָטַלְתָּי and קָטַלְתָּי forms, the 2ms plene forms appear in numerous texts, both early and late. Note especially the examples in Genesis, 1–2 Samuel, Isa 2:6 (see GKC §44g), as well as in pre-exilic inscriptional evidence (e.g., *yd'th* “do you (not) know” Lachish 3:8). (23) The explanation of the 2fs *qatal* conjugation forms in the scrolls reveals other problems with Qimron’s grammar, but these are addressed in the appropriate space below (in comments to p. 155, §C 2.1.1.1).

Similar assumptions about irregular spellings also underlie many of the observations about the language of the DSS. A case in point is

(22) Note the observations of Aaron Hornkohl (*Ancient Hebrew Periodization and the Language of the Book of Jeremiah: The Case for a Sixth-Century Date of Composition*, SSSL 74 [Leiden: Brill, 2014], 116 and 119) that the occurrences in Ezekiel and Jeremiah are likely due to Aramaic influence.

(23) See Sandra Landis Gogel, *A Grammar of Epigraphic Hebrew*, RBS 23 (Atlanta: SBL, 1998), 85

the interpretation of the gutturals and their apparent weakening. Qimron asserts (p. 99, §B 1) that “gutturals in DSS Hebrew had disappeared and were replaced by glides or by ultra-long vowels.” While this may be true for certain texts like 1QS and 1QIsa^a, based in part on the types of spellings described above, the vast majority of manuscripts do not exhibit these kinds of mistakes. If they do, it is never with the density one finds in 1QS and 1QIsa^a.

Moreover, such statements like this gloss over the fact that the loss or confusion of gutturals in the orthography is not uniform. It is, for example, quite unusual for *heth* to replace another guttural; again, this primarily occurs in 1QS and 1QIsa^a. (24) Why should this be the case? If all the gutturals had quiesced and were instead articulated as ultra long vowels or glides, *heth* should replace *heh* just as often as *aleph* replaces *heh*. Since it does not, it seems likely that *heth* was sometimes articulated as a true consonant, at least in some way, even if only in one register of the language and only by certain scribes. (25)

4.

Finally, the grammar frequently ignores the possibility that misspellings are due to graphic errors. Spelling mistakes inspired by slips of the eye and/or hand are not necessarily unrelated to pronunciation, but they can be. This ambiguity should, at the least, be recognized when information is derived from cases of haplography, dittography, and metathesis.

So, for example, the metathesis of maters around gutturals and *resh* is presented as evidence for the weakening of the gutturals, as it should be (p. 102, §B 1.1.3 and pp. 105–108, §B 1.2.3, 1.3.3, 1.4.3). Nevertheless, some of these instances may be due to graphic errors (e.g., בִּיחֲרִי “chosen ones of” 1QH^a VI, 21 for an intended בְּחִירִי [see p. 108, §B 1.4.3]), not necessarily related to the pronunciation of neighboring consonants. Examples of the metathesis among non-guttural letters include: וְקָלִי for an intended וְקָלִי “and are swift” (1QpHab III, 6 in a quotation of Hab 1:8: וְקָלִי “and are swift”); גִּזְעוֹ for an intended גִּזְעוֹ “its stock” (1QH^a XVI, 8); (26) מִתֵּךְ corrected to מִתֵּךְ “poured out” (1QH^a XXI, 25), reflecting what would be in the MT מִתֵּךְ (a *hophal*

(24) In addition to those examples listed in Reymond, “The Scribe of 1QS,” 246–48 and in idem, *Qumran Hebrew*, 103–104, 110, Qimron lists others (p. 104, §B 1.2.2). Some of these are certainly erroneous. See below.

(25) For more on guttural weakening, see Reymond, *Qumran Hebrew*, 71–114.

(26) Or, the first *waw* should be read as a *yodh* (as in H. Stegemann, E. Schuller, and C. Newsom, *1QHodayot^a, with Incorporation of 1QHodayot^b and 4QHodayot^{a-f}*, DJD 40 [Oxford: Clarendon, 2009], 216): גִּזְעוֹ for an intended גִּזְעוֹ.

participle of מוֹתֵךְ (with mater for short *u*); לִי for an intended לוֹ “to him” (4Q266 10 i, 9); יִקְפֹּץ corrected to יִקְפֹּץ “it will clench” (4Q418 88, 5); יָד for an intended יָד “his hand” (4Q511 10, 12). (27)

The dropping of a letter from the spelling of a word does not necessarily imply its quiescence or assimilation since many different letters are dropped. Note, for example, the frequency with which *daleth*, *lamedh*, and *nun* are dropped. (28) While Qimron (p. 95 n. 133, §A 9, as noted above) posits the spelling אִם “islands” (1QIsa^a at Isa 13:22) as evidence that the sequence *-iyyī-* had collapsed into an ultra long *-î-*, such a collapse seems unlikely in the case of the similarly misspelled הַיָּמִים *ha’yāmīm* “the days” (4Q252 IV, 2).

Furthermore, it is important to consider the possibility that words of a similar form influence each other in their spelling. Just among those listed above, note how many of the misspellings are sensible sequences of letters out of their specific contexts (e.g., וְקוֹל “and voice”; וְגוֹעַז “and its stem”; מִתּוֹךְ “from the midst of”; אֶכְשִׁיל “I will make stumble”; שְׂרֵי “princes of”; אִם “if”; הַמַּיִם “the water”). This seems unlikely to be sheer coincidence.

Such correspondences (referred to below as cases of “graphic assimilation” [see comments to pp. 66–70 §§A 3.2 and 3.2.1; pp. 118–19, §§B 6.1–2; and pp. 120–21, §B 6.3]) are important to note since they suggest that the orthography of the scribes was often influenced by factors other than phonology or, stated differently, that the graphic representation of one word might affect the graphic representation of another word, even when the two words did not correspond phonetically. Thus, certain spellings may reflect this tendency for graphic assimilation and not reflect aspects of grammar. For example, Qimron (p. 71, §A 3.2.2; p. 141, §B 12.2) suggests the spelling בֵּית “daughter of” 4Q76 at Mal 2:11 might reflect the pronunciation *bet* (based in part on the Greek transliteration βεθ), distinct from the pronunciation *bat* reflected in the Tiberian vocalization tradition. Instead, the spelling בֵּית likely reflects graphic confusion with the common word בֵּית “house.” Such confusion is especially likely given that the following word in Mal 2:11 is אֵל and the combination of words is then homographic with

(27) Some of the examples are drawn from Raymond, *Qumran Hebrew*, 25–27. Similarly, the misplacement of gutturals is not unlike the misplacement (i.e., metathesis) of non-guttural letters, like *šin*: אֶכְשִׁיל for an intended אֶשְׁכִּיל “I will consider” (1QH^a XVIII, 8); Stegemann, Schuller, Newsom (1QHodayor^a, DJD 40:237) note that a scribe indicated the *kaph* was an error, but he did not add further corrections. Note too שְׂרֵי corrected to רִשֵּׁי “heads of” (1QS^a I, 16) and בִּשְׁקָתִי for an intended בִּקְשָׁתִי “I sought” (4Q107 [4QCant^b] at Song 3:1 ≈ MT בִּקְשָׁתִי “I sought”).

(28) See Raymond, *Qumran Hebrew*, 32.

the place name, בית אל "Bethel." (29) Consequently, it is still quite possible that the word for daughter was pronounced *bat* not only in the Tiberian tradition, but also in the Hebrew of the DSS.

5.

The above criticisms are more of a conceptual nature and it is likely that many will hold differing opinions on these matters. However, there are also criticisms of the grammar that will elicit less disagreement. These relate to the basic mechanics of the grammar, including inconsistencies in the data and their representation, as well as mistakes in references and readings.

The first and most common group of errors has to do with cross-referencing. I have noted some of these at the end of this review, in an appendix. Assuredly there are many others. Such errors might be ameliorated by comprehensive indexing. Unfortunately, however, the two indices in the back of the book are not thorough. Second, although I have not done a thorough check of all the quotations, rather only a random selection, I was surprised to find there were several clear errors, as can be determined by a quick check of the online photographs (available, e.g., at <http://dss.collections.imj.org.il>). These are not cases that are difficult to read due to tears, breaks, erased letters, etc. or whose digital images are difficult to locate. These are words that can easily be read. At least some are significant for the arguments in which they appear (e.g., in relation to the quiescence of *heth*; see pp. 104–105, §B 1.2.2). Third, the labeling of sources is also sometimes incorrect.

pp. 17–31. There is no bibliography, which is a real deficit. While the "Abbreviations" contains many references to works cited in the grammar, it does not contain all of them. There are no references in the "Abbreviations" to the works by Martin Abegg; many of the works of Moshe Bar-Asher are not listed; the list of omissions could easily be extended. In addition, there is sometimes inconsistency between the abbreviations and how they appear in the footnotes. The text of 4QInstruction is listed as 4QhRazNihye in the list of abbreviations, but is elsewhere listed as 4QHokRazNihye (e.g., p. 108, §B 1.4.2 and 1.4.3). On p. 369 n. 2, §H 1.1 we find references to "M. Smith" and "van Peursen, pp. 136–157" but there are two works in the "Abbreviations" by Mark Smith and three by Wido van Peursen.

p. 33. The evidence for the language is drawn exclusively from the "the manuscripts found in the caves near Qumran" (p. 33). Nevertheless,

(29) Note that the reading of this phrase in 4Q76 at Mal 2:11 as בית אל נכר is mistakenly given as בית צין on p. 293 n. 17, §E 1.1.2.

sometimes Qimron cites scrolls from Masada (e.g., fragments of the Songs of the Sabbath Sacrifice on pp. 95–96, §A 9) even from a fragment of a scroll from the Schøyen collection (p. 118, §B 6.1), as well as from the medieval manuscripts of the Damascus Document (p. 118, §B 6.1). Unfortunately, the lack of a comprehensive index of cited sources means that it is difficult to know exactly how many times such sources are cited.

In the index of sources, no scrolls from Masada are mentioned at all. Why should these be excluded? Since the Ben Sira Masada scroll and the other scrolls from Masada seem to be from a similar milieu and time period as the DSS, they would seemingly offer more evidence for the same kinds of phenomena documented in the scrolls; their absence seems arbitrary. In fact, Ben Sira is thought to have been a Jerusalemite himself. Should his work not be at the foundation of Qimron's thesis on the Jerusalemite dialect of the scrolls?

Note, for example, the numerous features that would complement the grammar: The defective writing of a long *ū* vowel in צר “rock” (at Sir 40:15); defective writing of long *ō* (just after plene spelling) לדור ודור “generation after generation” (at Sir 44:14); *śin* appearing for etymological *samekh* in גא[שף] “one gathered” (at Sir 40:28); the possible **qatīl* base of אוינים “strength” (at Sir 41:2) and בויש “embarrassed” (at Sir 42:1); the elision of *aleph* in המרה “sight” (at Sir 41:18); the substitution of *‘ayin* for *aleph* in the exclamatory particle הע “hey!” for expected הא (at Sir 41:2) and in the word עדם “person” for expected אדם (at Sir 41:18, if עדם is not a simple mistake for עדה); the preservation of historical III-*yodh* in אתיות “those things coming” (at Sir 42:18); *heh* as a mater for the 3ms pronominal suffix: גדלה “his greatness” (at Sir 44:2). (30)

Yet another problem throughout the grammar that relates to its sources is the fact that Aramaic words from the DSS are often cited alongside Hebrew words, without any clarification that they are from a different language. So, for example, in the discussion of phonology, words from the Genesis Apocryphon are cited numerous times among other Hebrew words without remark (p. 110, §B 3.1; p. 111, §B 3.4; p. 115, §B 5.2.2; p. 124, §B 8.2.1). The citation of Aramaic is justified by the fact that the scribes of the Hebrew scrolls were likely reading and writing Aramaic too, but the Aramaic words should be isolated from the Hebrew and labeled clearly as Aramaic.

(30) See Eric D. Reymond, “New Readings in the Ben Sira Masada Scroll (Mas 1h),” *RevQ* 26/103 (2014): 327–43. Note that Qimron (p. 397, §H 1.8.2) does refer to the verse (Sir 40:14) where one finds the syntagm עם + infinitive construct (עם שאתר “when it lifts”), though the Ben Sira manuscript and the Hebrew phrase are not cited.

p. 42. Qimron notes that among the manuscripts of the Community Rule one may find “hundreds of cases of variant linguistic usages.” He infers, if I understand correctly, that the variation must be part of the textual tradition of the Community Rule and that one cannot use such variation, therefore, to argue that different morphologies or orthographies (say between 1Q8 = 1QIsa^b and 1QIsa^a) are reflective of different places of origin or of different groups of scribes. Nevertheless, this argument will likely only be convincing to those who already believe that the individual texts, like the Community Rule, were written in one place, by one group, at one time. The variations in the Community Rule manuscripts could also reflect, of course, the different dialects and idiolects of the scribes who copied the manuscripts in different places.

p. 46, n. 19, the reference to a *ואקטלה* form in 1Q8 = 1QIsa^b at Isa 48:18 is incorrect, the presumed reference is to Isa 57:18.

pp. 55–354, §§A–E. There is a general inconsistency with regards to how vowel length is indicated. Sometimes a macron is used to indicate a long vowel and sometimes not. For instance, Qimron writes: “. . . final *aw* was generally contracted to *ō* . . . *yado* ‘his hand’ (←*yadahū*) and *yado* ‘his hands’ (←*yadayhu*) . . .” (p. 72, §A 3.3). Similarly, §B 8.2.1 (p. 124) is labeled “*yī* → ‘*i*’” though the section concerns specifically the transformation of *yī*- into long *ī*. Similar inconsistencies between section labels and examples are found throughout the chapter on phonology.

The contraction of vowels and weak consonants sometimes results in ultra-long vowels, which are usually indicated with a circumflex, but there is again inconsistency and sometimes they are represented with a macron: “The original *ייה* . . . may well represent *yî* . . . as in the case of *יחי* (*yî* ← **yīhi*)” (p. 151, §B 16.1).

These inconsistencies, in turn, lead to confusion. For instance, is the connecting vowel *e* (between noun/verb and 2ms suffix [e.g., in *בעזוביכה* “when you abandoned” 4Q460 9 i, 8]) short or long? Although one may make an educated guess from terse explanations like that on p. 71 (third paragraph), §A 3.2.2 (31) that the *e* after singular nouns and infinitives construct is short, does this also apply to plural nouns with the same suffix? (32) One might infer that the same *e* vowel after plural nouns was long, given the etymology (i.e., *-ayka*), but some statements seem to suggest a common pronunciation for both singular and plural

(31) Qimron writes: “. . . distinction should be made between those cases where the *yod* is originally radical, namely *ē* ← *ay*, and those cases where it is merely a vowel letter representing *e* (← *i*)” (p. 71). This vowel is definitively characterized as short, but only much later (if I am not mistaken), on p. 141, §B12.2.

(32) On whether or not such forms even contained an *e* vowel of any kind, see below my questions in comments to pp. 265–69, §§D 2.0–D2.2.

nouns with the 2ms suffix. Note, for example, the statement: “The use of e.g. דבריכה for both the plural and the singular indicates that DSS Hebrew does not distinguish between such singular and plural suffixes” (p. 267, §D 2.2.1.2).

p. 65, §A 3.1, second paragraph, three lines up from the bottom. The Hebrew word is missing.

p. 65, §A 3.1, second paragraph, bottom line. The Hebrew word is missing.

pp. 66–70 §§A 3.2 and 3.2.1. Qimron lists words where sometimes the *e* vowel is marked with a mater and sometimes it is not. Those words that are written defectively are often homographic with other words. It seems likely that the different spellings of graphically similar words have mutually affected each other, something that Qimron himself (p. 82 n. 79, §A 4.5) recognizes for some words. (33) For example, the spelling of אמה (*’āmā^h*) “female servant” has likely encouraged a scribe to accidentally spell the word אימה (*’ēmā^h*) “fear” without *yodh*: אמה (*’ēmā^h*) “fear” 1QpHab III, 7 and IV, 7. Similarly, the spelling of אלים (*’ēlīm*) “gods, angels” encouraged a scribe to spell אילים (*’ēlīm*) “rams” without a *yodh*: אלים (*’ēlīm*) 11Q19 XVII, 15. Similar examples of graphic assimilation likely include: בנים *bānīm* “sons, children” and the spelling of בינים (*bēnaym*) “between” as בנים (*bēnaym*) 1QM I, 14, and passim; שבה *šābā^h* “she returned” and the initial spelling of שבה (*sēbā^h*) “old age” 1QH^a XII, 31; רקים *raqqīm* “empty” and the spelling of ריקים *rēqīm* “vanities” as רקים *rēqīm* 1QS X, 24. The reverse is also attested; a word in which we commonly see a mater influences the spelling of a word where we do not expect a mater (e.g., ואלינו *wa’elēnū* “and our God” 4Q460 10, 11 [see p. 67, §A 3.2]; והושעו spelled as if reflecting *wahōšē’ū* “deliver!” 1QIsa^a at Isa 45:22, corresponding to MT הוֹשִׁיעַ). This tendency for different words to be occasionally written in a similar way is akin to words spelled with the 3ms *heh* pronominal suffix being graphically similar to other words that end with *heh*; for example, שוכה “its branch” (Judg 9:49) matches שוכה “branch” (Judg 9:48); קצה “his end” (2 Kgs 19:23, = קצו in Isa 37:24) matches קצה, קצה, קצה “end.” (34)

(33) He notes the spelling of לנوم “to sleep” with an *aleph* לנאום in 1QIsa^a at Isa 56:10 as being influenced by the spelling of נאום “utterance of” and the spelling of ברוש “fir” with an *aleph* בראוש in 1QIsa^a at Isa 41:19 as being influenced by ראוש “head.”

(34) See Eric D. Reymond, “The 3ms Suffix on Nouns Written with a *Heh* Mater,” in “Like ‘Ilu Are You Wise’: Studies in Northwest Semitic Languages and Literatures in Honor of Dennis G. Pardee, eds. H. H. Hardy II, Joseph Lam, and Eric D. Reymond (Chicago, Oriental Institute, forthcoming) and idem, “Reflections on Orthography and Morphology in Ben Sira’s Hebrew: The 3ms *Heh* Pronominal Suffix,” in *Seventh*

p. 72, §A 3.3. Note the inconsistent representation of vowel length: “. . . final *aw* was generally contracted to *ō* . . . *yado* ‘his hand’ (←*yadahū*) and *yado* ‘his hands’ (←*yadayhu*) . . .” See the comments above to pp. 55–354, §§A–E.

p. 89, §A 5.2. Qimron is right that it is not uncommon to find the *heh* marking a final *ō* vowel. Further possible examples of the 3ms *heh* suffix are listed in a forthcoming article (35) and include: the phrase מראתה בנוצתה “its crop and its feathers” 4Q24 (4QLev^b) at Lev 1:16 (≈ MT מארתו בנוצתו); (36) also עלמה *‘almō* “his young man” in 4Q52 (4QSam^b) at 1 Sam 20:38 (≈ MT הנער); and perhaps [ח]ילה “his [i.e., Manasseh’s] army” in 4Q169 3–4 iii, 11 and [מ]לחמתה “his [w]ar” in the same line. (37)

p. 94 n. 129, §A 8. The reference to Reymond should have the page numbers 42, 62.

p. 95, §A 8. Two possible examples of a consonantal *waw* written twice are quoted at the end of this section, without source citations. The first reference should be: שו “nothingness, vanity” in 1QpHab X, 10, 11 (≈ MT שו). In this manuscript the *waw* and *yodh* are indistinguishable (note the spelling of יום “day” in 1QpHab XI, 7 where the *yodh* is longer than the following *waw*). Elsewhere, Qimron reads the same word שוי (e.g., p. 78 n. 68, §A 4.1; p. 82, §A 4.4; p. 131 n. 119, §B 8.4.4); the possible reading שו is not noted in the indices, where we find (p. 510) instead the listing “שו (שוא)” and a reference only to §A 4.4.

International Symposium on the Hebrew of the Dead Sea Scrolls and Ben Sira, Strasbourg, June 22–25, 2014, STDJ 124 (Leiden: Brill, 2018), 226–44.

(35) Reymond, “The 3ms Suffix on Nouns Written with a *Heh Mater*,” forthcoming.

(36) For this last example, see Ian Young, “Observations on the Third Person Masculine Singular Pronominal Suffix -H in Hebrew Biblical Texts,” *Hebrew Studies* 42 (2001): 225–42 (esp. 234–35). Less convincing are the other examples he cites. He suggests אתה “it” 4Q25 (4QLev^c) at Lev 4:14 for MT אתו and SP אתו. In this case, the antecedent is either bull, שור, or sin-offering, חטאת, the latter of which is obviously feminine. Young cites another possibility of בלה in 1QIsa^a at Isa 30:5 for MT בל, though here the consonants could just as easily be construed as the noun “destruction,” and the whole clause translated: “destruction by fire (will be) on account of a people . . .” Young (“Observations,” 237–38) notes that the *heh* of וכתה in 4Q67 (4QIsa^a) at Isa 58:13 for MT וכתה could be construed as another example, though it is also possible to read it as a 3fs suffix.

(37) Nevertheless, others, like Shani L. Berrin (*The Pesher Nahum Scroll from Qumran: An Exegetical Study of 4Q169*; STDJ 53 [Leiden: Brill, 2004], 279), Mauryan P. Horgan (“Nahum Pesher (4Q169 = 4QpNah),” in *The Dead Sea Scrolls, Hebrew, Aramaic, and Greek Texts with English Translations: Pescharim and Other Commentaries and Related Documents*, ed. James H. Charlesworth, PTSDDSP 6B (Tübingen: Mohr Siebeck, 2002), 144–55 (esp. 153), DSSSE, DSSR, Eduard Lohse (*Die Texte aus Qumran, Hebräisch und deutsch* [München: Kösel, 1964], 266) see the suffix on [ח]ילה and [מ]לחמתה as 3fs.

The source for the second possible example, עשו "Esau," is a bit more difficult to find. The name seems to be spelled with a misplaced *yodh*, עשי, in 4Q252 4, 1 for an intended עיש* [?] (the reading being based on the relatively distinct shapes of the *yodh* and *waw* in this manuscript). It seems more likely that Qimron has intended reference to עישו in 4Q215 1–3, 7 (where the *yodh* and *waw* are again relatively distinct).

p. 100, §B 1.1.1.1. Both the reading and the source citation of the first word listed from Peshar Habbakkuk are incorrect. Qimron writes: "ויספהו" but it should be "ויספהו" (which presupposes the elision of the initial *aleph* of אסף "to gather"). The citation given to this is "1QpHab v 4" but it should be "1QpHab v 14." Qimron's mistaken reading suggests another way that the word in the passage might have been construed: ויספהו "and he will end him" (i.e., according to Tiberian vocalization: וִי־סָפְהוּ from סָפַח).

The reading אשופה (in 1QIsa^a at Isa 42:14) is at least plausibly explained as from נשף "I will blow" rather than as an error for אשאף "I will pant." This would give the passage a more menacing tone.

No source citation is given for שפאו and the cross-reference (B 11.3) is incorrect. The citation is 4Q70 at Jer 14:6 and the cross-reference should be B 1.1.3.

p. 101, §B 1.1.1.3. The label to this section is wrong. It reads CVC - 'V → CV - 'V[^] [my word processor does not allow the circumflex to go over the V] but it should read instead CVC - 'V → CV - CV[^].

Here, as elsewhere, the standard indication of stress in the transliteration of Samaritan words is the superscript vertical bar: '. In contrast to how this symbol is usually used in the grammar, the mark here seems to appear after the stressed syllable (not before).

p. 101, §B 1.1.2. As examples of the interchange of gutturals, Qimron notes the writing ועתה for an intended ואתה "and you" in 4Q491 8–10 i, 8 and in 4223–224 2 v, 5. Nevertheless, the significance of these examples is diminished since the two words commonly occur in the same syntactic position (at the head of a clause) and would naturally (even when articulated "correctly") be confused by speakers. (38)

p. 102, §B 1.2.1.1. The word הִזְקִיתִּיכָה "I hold you tight" (1QIsa^a at Isa 28:23) is not an example supporting the shift ChV → CV[^] since no consonant precedes the *heh*. It deserves its own section, presupposing the development hV → V[^]. This, in turn, would presuppose that an ultra long vowel might go unmarked at the beginning of a word. For another possible example, see the following note. All things considered, I think it more likely that the sequence hVGuttural has developed

(38) See Raymond, *Qumran Hebrew*, 76 and the literature cited there.

into GutturalV⁻ or GutturalV[^] in the dialect reflected in 1QIsa^a (where one may assume a general coalescence of all the gutturals). That is, *hīḥzaqtikā* developed into *ʾīzaqtikā* or *ʾīzaqtikā*.

p. 102, §B 1.2.1.2. The word הַאֲזִינוּ “give ear!” (1QIsa^a at Isa 28:23) is not an example supporting the shift CVh → CV[^] since no consonant precedes the *heh*. See the comment above. Assuming the coalescence of gutturals, we might expect *haʾzīnū* → *ʾāzīnū* or *ʾāzīnū*.

p. 103, §B 1.2.1.3, third paragraph, second to last line. The label of the Hodayot text is incorrect. It should be 1QH^a XIII, 35.

p. 104, §B 1.2.1.4. The omission of the definite article is not articulated in relation to a sound rule or phonetic development as preceding paragraphs. It would seem to imply the same development mentioned above in relation to p. 102, §B 1.2.1.1, namely: hVGuttural has developed into GutturalV⁻ or GutturalV[^].

Qimron lists הטל הראשון from 1QM VIII, 15 as an example of the omission of the definite article. First, this is an incorrect reading. The online digital photograph (dss.collections.imj.org.il) shows clearly הטל הראשון. Second, the definite article has not been omitted. The case of המנחה חדשה from 11Q19 XIX, 11 seems like a possible case of haplography. The following example, אחרת from 11Q19 XLIII, 5, may also be a case of haplography (the preceding word is לשונה) or perhaps the preceding word is indefinite (cf. the phrase זים ליום Ps 19:3).

pp. 104–105, §B 1.2.2. Qimron lists some readings of words spelled with *heth* for *heh*, though these are erroneous. For example, he cites ינהלם in 1QIsa^a at Isa 49:10, corresponding to MT ינהלם. Nevertheless, the digital photos online (dss.collections.imj.org.il) clearly show a *heh*. Similarly with ואשיתחו in 1QIsa^a at Isa 5:6 (which is actually ואשיתהו in 1QIsa^a, as in the MT) and הפחת in 1QIsa^a at Isa 24:18 (which is actually הפחת in 1QIsa^a, as in the MT). These are not corrected spellings in the manuscripts, but rather simply misreadings in the grammar. I also am skeptical about some of the forms Qimron recognizes as corrections. These may not really be examples of corrections, but simply awkwardly formed *hehs*. E.g., the relevant letter of מהול (in 1QIsa^a at Isa 1:22) has a longer right leg than left, as we would expect for a *heh*. Although this *heh* does not have the characteristic long top bar with a long tail to the left (after the left leg), this is not unique. Notice the *heh* of יהי at Isa 1:18 three lines above; it looks very much like the *heh* of מהול. (On the other hand, the *heh* of ויתהללו does seem to have been corrected from an earlier *heth*; notice that the citation is again incorrect: instead of Isa 46:25, it should be 45:25). In addition, it should be remembered that the similarity in shape between the two letters is likely at the root of some of the confusion, especially where a scribe seems to have caught the mistake and corrected it.

p. 105, §B 1.3.1.1. Note the interesting suggestion of taking תביט “you look” in 4Q51 [4QSam^a] at 1 Sam 2:29 (which agrees with the LXX) as תבוט for an intended form with ‘*ayin*, presumably with a *u* theme vowel like Aramaic תבעוט “you reject” (cf. MT תִּבְעֹטוּ and the sense in the Targum).

p. 106, §B 1.3.1.3. The paragraph is labeled $V^{\circ}V \rightarrow V^{\circ}$, yet the two examples of מערכת (if singular: *ma‘rakat* > *ma‘rakat* > *mâkat*; if plural *ma‘rakôt* > *ma‘rākôt* > *mākôt*) in 1QM V, 3 and VI, 5 seem like they go with the preceding paragraph, illustrating $CV^{\circ}rV \rightarrow CV^{\circ}$ or $CV^{\circ} \rightarrow CV^{\circ}$ (unless Qimron is assuming מערכת had its Tiberian form, e.g., מעֶרְכַּת).

The example of ונודעה in 4Q381 48–50, 7 may be a case of hyper-correction (for ונודע) and thus exemplifies the degree to which the scribes had lost track of the spelling of words with gutturals. Nevertheless, one wonders if this might be a case of misdivision of words, for an intended ונודע האלהים? The similar examples listed in the following section, §B 1.3.3, suggest the former explanation. These other examples should be listed in this paragraph (i.e., §B 1.3.1.3), not where they are listed in the paragraph that details the “Misplacement of ‘[*ayin*]” (i.e., §B 1.3.3).

p. 106, §B 1.3.2. Qimron lists as an example of ‘*ayin* replaced by *aleph* the form וכארום from 4Q169 3–4 iii, 4, which he translates “repulse” and notes parenthetically “also in Mishnaic Hebrew and Aramaic.” The word, of course, seems to be a byform of כער and its listing here would seem to contradict Qimron’s statement concerning which roots are considered in this section (p. 99 n. 2, §B 1): “Substitutions of roots such as . . . כער / כאר are not restricted to DSS Hebrew and are therefore are [sic] not listed.”

The writing of ואתה for an intended עתה “now” is not exclusively related to the guttural letters, as noted above (in reference to p. 101, §B 1.1.2).

p. 106, §B 1.4.1. The reading צִצְחוֹת in 1QIsa^a at Isa 58:11 should be צִצְחוֹת according to the online digital photos. There is no correction.

Another example of the omission of the phoneme *h* might be נ[כח] in 2Q18 at Sir 6:22 which corresponds with נְכוּחָה in Ms A (*kethib* = “obvious” and *qere* = “justified”).

It seems significant that among the words listed where the *heth* was not written, all but one have been corrected by scribes to include the *heth*. Furthermore, note that five of the eleven cases involve a spelling where *heth* and *heh* abutt, allowing for the possibility of a graphic mistake due to the similarity in shape between the letters. These factors suggest to me that the sound represented by *heth* had not entirely elided, but, in fact was still perceptible to most scribes. (39)

(39) See the analysis in Reymond, *Qumran Hebrew*, 107–11.

p. 109, §B 1.4.3.1. Qimron writes: “A distinction should be made between phonological scribal errors such as those involving gutturals and accidental scribal errors. In principle the former is more likely than the latter.” Why would errors caused by phonological factors occur more often than errors caused by graphic factors? No justification for this assertion is given. It seems both might occur with equal frequency and often be interrelated. Descriptions of such text-critical matters should be included in a grammar of this kind.

p. 109, §B 2. The paragraph lists forms that do not exhibit the assimilation of vowels to gutturals (e.g., /i/ > /a/ before ‘ayin). While it may be the case that הופיע in 1QM XVIII, 10 is an imperative (perhaps pronounced ‘ōpê [< hōpe‘]) and not a qatal conjugation form (i.e., ‘ōpî [< hōpî‘]), such forms are exceptions and not the rule. A better example, not listed in the grammar, may be הושיע “deliver!” (4Q72 at Jer 31:7 ≈ MT הושע), though this could also be a qatal conjugation form. All the same, the hiphil imperative commonly occurs without a yodh mater (e.g., והקם in 1QS XI, 16). So, in forms like והושע “and save!” (4Q381 15, 2) it is unclear whether or not the historical *i vowel was realized as ê or a.

Qimron also includes forms like באופיע “when (lights) shine” from 1QS X, 2 and בהגיע “when they arrive” from 4QM [sic! for 1QM] XVII, 11, which are similar in their graphic form at least to MT בקריע “when (a person) shouted” 2 Chr 13:15; ובגגיע “when it arrived” Est 2:12. In these cases, both the DSS and MT forms do not assimilate *i to a. Nevertheless, note that the yodh mater in the infinitive construct is uncommon in the DSS (see pp. 176–77, §C 1.7.1), where one finds defective forms like בהופע in 4Q258 VIII, 12. In such forms, the lack of yodh does not necessarily presuppose the assimilation of the vowel to the guttural ‘ayin, rather it may reflect simply the vowel e, ē, or ê.

p. 112, §B 4. The misspellings of various sounds are listed (e.g., kaph for what should be gimmel and vice versa; daleth for what should be tau and vice versa). No further significance of these misspellings is noted, though they seem to demonstrate how misspellings do not always represent phonological shifts. One can infer, for instance, that just as kaph could be spelled by accident with a gimmel without implying the coalescence of velar phonemes, so an aleph might be written for what should be a heh without presupposing coalescence of glottal phonemes.

pp. 118–19, §§B 6.1–2. Graphic assimilation may also be at play in those cases where the historical s (i.e., the sound represented by samekh) is written with a šin/šin letter. For example, it seems likely that the name מנשה “Manasseh” has led to what should be מנסה “is testing” being written מנשה (in 11Q19 LIV, 12). Similarly, due to the frequency

of the verb שִׁית “to set” what should be יִסִּיתָךְ “he will mislead you” is written יִשִּׁיתָךְ (in 11Q19 LIV, 19); the name אֵנוֹשׁ “Enosh” and its homonym “person” likely influenced the spelling of אֵנוֹס “one forced” as אֵנוֹשׁ (in 1QS VII, 12); the spelling of the words שֵׁעָרָה “hair” and שֵׁעָרָה “barley” likely played a role in the spelling of סְעָרָה “storm” as שֵׁעָרָה (in 4Q88 at Ps 107:29).

pp. 120–21, §B 6.3. Qimron attempts to explain the spelling of *s* (i.e., the sound represented by *samekh*) with the *šin/šîn* letter by positing two explanations: 1) the phonemes *s* and *š* had coalesced or 2) that scribes mistakenly thought words with *samekh* should be written with the *šin/šîn* letter and so hyper-corrected their spellings accordingly. He offers various arguments why explanation 2 is invalid. He suggests that the very frequency of these spellings negates the explanation of hyper-correction and that the scribes of the “non-formal manuscripts whose orthography is extremely phonetic” would not be concerned about such “historical orthography” and would not, therefore, hyper-correct such spellings. Of course, there is a third explanation for these spellings that is unstated, namely that the *šin/šîn* letter had two values (*s* and *š*) and that since the value *s* was also represented by *samekh*, scribes wrote the *s* phoneme sometimes with *samekh* and sometimes with *šin/šîn*. There is no reason to presume anyone believed that the verb “to test” was etymologically *nśy* (instead of *nsy*) and so wrote *šin/šîn* instead of *samekh* in order to reflect this misconceived history. Moreover, in quite a few cases, the spelling of the word with *šin/šîn* can be partially explained by the process of graphic assimilation, as explained above.

p. 124–25, §B 8.2.1. The section title, “*yi* → ‘*i*,’” and the phenomenon it describes, transformation of *yi*- into long *ī*, are inconsistent in regards to the representation of vowel length. See comments to pp. 55–354, §§A–E.

The second sentence of the paragraph is just a fragment and is missing its predicate. It should read at its end “. . . see B 8.2.2.”

Noting the relative frequency of a single *yodh* to represent the historical sequence *yi-* in I-*yodh* verbs (i.e., יִרָא for יִירָא “he will fear”), Qimron suggests that this orthography may represent the shift of *yi-* to *ī-*. This is entirely possible for a subset of the scribes. However, three arguments point against this as a universal phenomenon for all scribes in their literary register. First, none of the forms listed attests an initial *aleph*. If such a shift had taken place, would we not expect to see such spellings? That is, third person forms that look like 1cs forms (e.g., אִירָא “he will fear”)?

Second, the counter examples (where we do see two *yodhs* in sequence) are numerous enough that one must consider that at least

some scribes knew the “correct” pronunciation and continued to articulate the initial *yodh* in their recitation of texts. (40)

Third, some evidence cited seems ambiguous. Qimron observes that the scrolls seem to show a *yodh* mater consistently in the forms that begin with *taw* (i.e., 3fs, 2ms / fs, 2mp / fp), but not in those that begin with *yodh*, inferring that this is evidence for the initial *yodh* marking the initial ’ī- vowel. But, the same dichotomy is reflected in the consonantal text of the MT where the Tiberian vocalization tradition suggests no difference in sound (e.g., יָרְאוּ “they will fear” Qoh 12:5 vs. תִּירְאוּ “do [not] fear” Neh 4:8; וְיֵטֵב “and it will be good” 2Kgs 25:24 vs. וְהֵיטֵב “and it will be good” Ps 69:32). While one might view these spellings as evidence that the same shift (yī- to ’ī-) also underlies the consonantal biblical tradition, this seems to stretch the evidence too far. In the end, it seems more likely that it was simply common for both historical I-*yodh* and I-*waw* roots to be spelled defectively in *yiqtol* forms.

p. 125, §B 8.2.2. The title to the paragraph should really read “iyi (iye or eye) → î, î’i, (e’e or ê)” to account for the example וַאשׁ from 4Q176 8–10, 13 (for an expected וישׁ).

p. 131, §B 8.4.5. Qimron writes in relation to the contraction of -*ihū* (יְהוּ) to -*iw* (יְ-) that “In DSS Hebrew only the form with *hé* occurs (e.g., שְׁמַרְתִּיהוּ, אֲבִיהוּ, see §D 2.3.4).” In a footnote he adds: “The only exceptions are in biblical and apocryphal texts.” I am not sure what he means by “apocryphal,” but perhaps it is Ben Sira (?). In any case, these statements are contradicted by §D 2.3.4 (p. 272) where he lists the following examples of the shorter suffix: אֲבִי in 4Q225 2 ii, 4; אֲחִי in 4Q251 17, 3. Other examples, not listed, include אֲבִי in 4Q416 2 iv, 1; 11Q19 LXIV, 2; 1QIsa^a at Isa 3:6; 8:4; 22:23, 24 and elsewhere in the biblical scrolls; אֲחִי in 1QIsa^a at Isa 9:18; 19:2 and elsewhere in the biblical scrolls; פִּי in 1QH^a X, 19 (written פִּי due to haplography); (41) 4Q381 1, 3. (42) Among the non-biblical scrolls, it might be mentioned, biblical phraseology seems influential primarily in the cases from 4Q416 and 11Q19. Most, if not all, of the instances in the biblical scrolls correspond (not surprisingly) to forms with the shorter suffix in the MT. (43)

(40) Qimron lists five counter examples, but additional ones can also be found: יֵטֵב in 4Q521 2i+3, 7; יִינְקוּ in 11Q5 XXII, 5; יִירְשׁוּ in 4Q385 4, 3; 1QIsa^a at Isa 54:3; 1QIsa^b at Isa 54:3, 60:21.

(41) See Stegemann, Schuller, Newsom (*1QHodayot*⁴, DJD 40:138); Qimron, *The Dead Sea Scrolls: The Hebrew Writings*, 1:70–71.

(42) For more statistics, see Reymond, *Qumran Hebrew*, 143 and nn. 303, 304.

(43) For some statistics, see Reymond, *Qumran Hebrew*, 142 n. 302.

In relation to the 3ms object suffix on 1cs *qatal* conjugation verb forms, it is inaccurate to suggest that the יהו- suffix (i.e., שמרתיהו) totally (or even nearly) eclipses the י- suffix (i.e., שמרתי). In fact, it seems that both are about equally attested among the scrolls in general (considering both biblical and non-biblical scrolls together). According to my search of Accordance, the long form יהו- occurs twenty times in total (in ten separate manuscripts) and the contracted form י- at least fifteen times (in at least seven separate manuscripts). As for the shorter, contracted form, Qimron cites in §D 2.3.4 (p. 272) נתתי from 4Q522 9 ii, 11. Other examples of the contracted form include the defective spelling ונתתו in 4Q388a 7 ii, 5 (unless this should be construed as ונתתי); among the biblical scrolls, note especially those of 1QIsa^a: והלבשתי at Isa 22:21; ותקעתי at 22:23; והפלתיו at 37:7; שלחתי at 55:11. (44) Among these biblical scrolls, the MT has the shorter suffix (with only one exception).

In the end, the statement “Thus, in the Biblical texts, the suffix יהו- almost universally replaced the suffix י- in the corresponding MT passages” (p. 272, §D 2.3.4.1) is simply wrong. Nevertheless, part of Qimron’s larger point is true, that is, the יהו- suffix on nouns is more common among the non-biblical scrolls and less common among the biblical scrolls. This longer form of the suffix seems to show an ascendancy in later Hebrew writings in general.

p. 141, §B 12.2. Note that Qimron groups בית “daughter” from 4Q76 at Mal 2:11 with other examples where the stressed historical short /i/ is marked by a *yodh* (p. 141), but בית is really a construct form, “daughter of,” and is, therefore, not stressed. Moreover, as explained in section 4 above, the likelier explanation is that the word for daughter was spelled like the word for house due to graphic assimilation.

p. 144, §B 14.3. In suggesting why *hiphil yiqtol* forms like יבדילי imply general penultimate stress, Qimron mentions that the “*yod* (unlike *waw*) never represents a short vowel.” An extra phrase should be added to the end of that sentence “. . . in such *yiqtol* forms.” Otherwise, this creates a contradiction with the form he cites immediately afterwards: בעוזביכה “when you abandoned,” since on p. 141, §B 12.2, he characterizes the *yodh* in this and similar forms as marking a “short and stressed” *e* vowel.

p. 151, §B 16.1. There is an inconsistency in the way that vowel length is represented, especially when this is a result of contraction of

(44) Note also the following examples drawn from Accordance: צייתי in 4Q33 (at Deut 18:20); מאסתיו in 4Q52 at 1 Sam 16:7; בראתי in 1Q8 at Isa 43:7; צרתי [י] (idem); עשיתי (idem); בראתי in 1Q8 at Isa 45:8; הבאתי in 4Q58 at Isa 48:15; קראתי in 1Q8 at Isa 51:2; נתתי in 1Q8 at Isa 51:2; רפתי in 4Q58 at Isa 57:19; נתתי in 11Q4 at Ezek 4:6.

vowels and weak consonants: “The original ייה . . . may well represent *yī* . . . as in the case of יחי (*yī* ← **yihī*).” See comments to pp. 55–354, §§A–E.

p. 155, §C 2.1.1.1. In relation to 2fs *qatal* conjugation forms in the scrolls, Qimron writes that the 2fs *qatal* forms do not appear in the non-biblical texts. This is true, if one categorizes the pesharim as biblical. In the Peshier to Nahum (4Q169) there are two examples of this form, though these verb forms do not match those of the MT. Both verbs are spelled with *-t* (גלית “you will reveal” and והראית “you will show”) in 4Q169 3–4 ii, 10–11, in a quotation of Nah 3:5, where the MT has 1cs forms: וגליתי “I will reveal” and והראיתי “I will show.” Given the fact that the prophet in the biblical text is clearly addressing a feminine entity, presumably the city (based on the 2fs pronouns like אַלֶּיךָ), it seems reasonable that the two forms spelled with *-t* should be interpreted as 2fs. The text from which the pesharist was working presumably reflects an early attempt at sanitizing the text and removing the reference to God undressing someone (even if this was only a subconscious reaction of a scribe who read וגליתי as 2fs): “You shall hoist [your] skirts up to your face and show the nations [your] nudity” (DSSSE). (45)

Moreover, the only 2fs *qatal* conjugation forms that evidence the *-ty* ending in the other so-called biblical scrolls occur in 1QIsa^a. The same scroll witnesses 2fs *qatal* conjugation forms that end in *-th* (see p. 139–40, §B 12.1); Qimron infers that all these reflect a single pronunciation *-e* (p. 139, §B 12.1). While this seems plausible, it does not necessarily mean that the underlying phonological shift of /i/ to /e/ in final unstressed syllables was universal in the Hebrew of the scrolls.

pp. 157–59, §C 2.1.2.1. Qimron describes the paragogic *nun* as “practically unused” in DSS Hebrew (p. 157). Note the discussion above in section 1. Here, one senses that the lack of distinction between a colloquial and literary register confuses the description. He lists ten words attesting the paragogic *nun* from the non-biblical scrolls, though he describes these words as either “linked to biblical verses” or “based on biblical verses.” In addition to these, he cites another four doubtful cases. Accordance identifies still other examples, including: תנוסוך “you will flee” (4Q163 23 ii, 5 [Qimron only lists the occurrence of the same form in line 7]); [י]קומוך “they will arise” (4Q174 6–7, 6); ידעוך “they will know” (4Q381 1, 2); ישובוך “they will return” (4Q419 8 ii, 8); ישובוך “they will return” (4Q525 20, 8).

(45) For a different interpretation, see Berrin, *Peshier Nahum Scroll from Qumran*, 256.

Qimron (p. 159, §C 2.1.2.1.2) notes the surprising frequency of the paragogic *nun* in the biblical manuscripts, noting that it even occurs in cases where the MT lacks the morpheme. He suggests that in these cases it may be original to the text (especially when the paragogic *nun* is found in other traditions like the Samaritan Pentateuch). Yet, the evidence is rather uneven and inconsistent.

Given the numerous examples, it seems that this peculiar morpheme should not be described as “practically unused” in DSS Hebrew. All things considered, it seems to be used quite a bit, though it was likely not part of the colloquial register.

p. 157 n. 13. Reference could be made to Tamar Zewi, *A Syntactical Study of Verbal Forms Affixed by -n(n) Endings in Classical Arabic, Biblical Hebrew, El-Amarna Akkadian and Ugaritic*; AOAT 260 (Münster: Ugarit-Verlag, 1999).

p. 158, §C.2.1.2.1 (second paragraph). The reference to Deut 28:16 should be Deut 28:15.

pp. 160–65, §C.2.1.3. The brief description of Proto-Semitic verbs and their ultimate realization in Second Temple Hebrew seems somewhat unclear to me. To take just the most relevant item: on p. 161, the grammar states in relation to *yiqtol* verbs in Second Temple texts:

What does in fact indicate that the modal use is weakened is the fact that **וַיִּקְם** and **וַיִּקְטֹּל** denote not only the modal imperfect but also the indicative imperfect.

Clear enough. The form **וַיִּקְם** is shorthand for the *waw* conjunction plus the *hiphil* 3ms/fs and 2ms short-*yiqtol* conjugation (i.e., the jussive/preterite) as well as *qal* 3ms/fs and 2ms short-*yiqtol* forms from II-*yodh* roots (e.g., MT **אַל-נָא תָשֶׁת** “do not set” Num 12:11); the form **וַיִּקְטֹּל** is shorthand for the *waw* conjunction plus the 1cs cohortative. The modal or volitive (i.e., jussive/cohortative) sense of these forms is inferred to have been lost since the same forms often represent a future indicative mood. This latter usage is, of course, unusual and stands out as an idiosyncrasy of the DSS. Nevertheless, the statement above is not immediately backed up by clear examples where **וַיִּקְם** or **וַיִּקְטֹּל** represent such an indicative sense. Moreover, one wonders if the indicative nuance to such forms is dependent on the presence of a preceding *waw* conjunction. Is it possible that **וַיִּקְם** alone is used to represent the future indicative too?

The grammar proceeds with two lists. The first is titled “Forms Not Preceded by *Waw*.” Among the short-*yiqtol* forms included in this list, only verbs following the negative particle **אַל** are listed (with one exception: **וַיִּסַּף** from 4Q418 137, 2, which is translated as though it were

a jussive: “let him add” [p. 162], though the context is broken). Outside this one exception, no explicit reference is made here to the six forms listed by Martin Abegg as short-*yiqtol* forms (not preceded by a *waw* conjunction or the particle אל “not”): יהי “it will be” (1QS II, 13); ימה “he will wipe out” (1QS XI, 3); יעל “it will send up” (1QH^a XVI, 26); יכה “he will reprove [?]” (4Q372 18, 2); תוסף “you will add [?]” (4Q418 199, 2). (46) The last two examples are from broken contexts and it is hard to be certain of their nuance, but the first three seem to express a future indicative sense. It seems to me that the omission of these forms obscures the point that the grammar here is attempting to make, that verbal forms previously marked as indicating a volitive sense (i.e., a jussive/cohortative sense) are used as future indicatives in the DSS. The omission of these forms is perhaps connected to the assertion (made on pp. 371–72, §H 1.2) that such short-*yiqtol* forms tend to occur at the beginning of sentences. The first three of the above examples noted by Abegg are all exceptions to that rule and are cited in a footnote (pp. 371–72 n. 7) together with other possible counter-examples to the clause-initial rule. For more on the distribution of such forms and questions on the syntax involved, see the comments below to pp. 371–72, §H 1.2.

The second list is titled “The Imperfect with *Waw*.” Among the short-*yiqtol* forms listed here, it is not made explicit which verbs refer to the past (and should thus be construed as *wayyiqtol* forms, e.g., ותעמד “and you established” 1QH^a X, 10; ותופע “and you appeared” 1QH^a XII, 24); which forms reflect volitive nuances (and should thus be construed as jussive forms, e.g., . . . ואומרים יברככה . . . אנשי גורל אל . . . ואומרים יברככה . . . ויאר לבכה “the people of God’s lot . . . who say . . . ‘may he bless you . . . and may he guard you . . . and may he enlighten your mind’” 1QS II, 2–3); (47) and which simply refer to the future (and should thus be construed as *waw*+short-*yiqtol* [see below for examples]). It is only this last category which is unusual and unlike what we expect based on the MT and the Tiberian vocalization tradition.

The grammar alludes here to only one case where a form like יקם represents a future indicative nuance, and the reference occurs in a footnote (p. 163 n. 26) without even citing the Hebrew word. The form is presumably ויקטר “and he will burn” (i.e., the simple *waw* conjunction plus the *hiphil* short-*yiqtol* = ויקטל with the sense of ויקטיל) in

(46) Martin G. Abegg, “The Hebrew of the Dead Sea Scrolls,” in *The Dead Sea Scrolls after Fifty Years: A Comprehensive Assessment*, ed. Peter W. Flint and James C. VanderKam, 2 vols. (Leiden: Brill, 1998–1999), 1:325–58 (spec. 336).

(47) Note the similar expression with a volitive ויאר in 11Q14 1 ii, 7. In 4Q219 II, 29 one finds ותצלח “so you will succeed,” a *hiphil* short-*yiqtol* with purpose-result sense (following a presumed imperative in the lacuna).

11Q19 XXIII, 16 (though Qimron labels this line 17). (48) Why are further examples of this phenomenon not isolated? Note, for example, *יבוא ברית אל . . .* “he will withdraw my soul from the pit and steady my steps on the path” (1QS XI, 13); *ויקם* “he will enter the covenant of God . . . and will establish . . .” (1QS V, 8).

Perhaps one reason more examples with this future indicative sense are not listed is that many of the *w*+short-*yiqtol* forms are from poetic texts (like the Hodayot) and/or are from broken contexts and, subsequently, their exact nuance is hard to pin down. At the least, it should be mentioned that many of the *w*+short-*yiqtol* forms listed seem to reflect a standard syntax and grammar, familiar to us from the MT / Tiberian Hebrew vocalization tradition. Specifically, many seem to be *wayyiqtol* forms:

אתה בראתה רוח בלשון ותדע דבריה ותכן פרי שפתים בטרם היותם ותשם
דברים על קו

You created the spirit on the tongue, you know its words, you established the fruit of lips before they were, you set words on the line . . . (1QH^a IX, 29–30).

ברוך את[ה] אדוני כי אתה פעלתה אלה ותשם בפי עבדכָה הִדֹּת ת[ה]ל[ה]

Blessed are you, Lord, for you made these things and you set in the mouth of your servant songs of praise (1QH^a XIX, 35–36).

The number of short-*yiqtol* forms preceded by *waw* that clearly indicate a future indicative sense are not infrequent in the scrolls as a whole, but they do not make up the majority of such forms. Furthermore, they are concentrated in the Community Rule, Hodayot, and Temple Scroll. In addition to those listed above, other relatively clear examples include: (49) *ויז* “and he will sprinkle” (1QS IV, 21); *ויה* “and (his counsel) will be . . .” (1QS VI, 22); *ותעש* “and you will do . . .” (1QM XI, 9); *ויהם* “and (the lofty abode) will roar [if the verb is הום and not המם “move noisily”] . . .” (1QH^a XI, 35); *ותכרת* “and you will destroy”

(48) In Qimron’s edition (*The Dead Sea Scrolls: The Hebrew Writings*, 1:162), as elsewhere, it is labeled line 16.

(49) Such forms are easiest to discern in the context of other *yiqtol* forms, as with *ויהי* “they will send and (its trunk) will open . . . and it will be . . .” (1QH^a XVI, 9); *לוא ישמע ויעש* “(the person who) will not listen and will act (arrogantly). . .” (11Q19 LVI, 8); *ישמור ויעש* “he will guard and do . . .” (11Q19 LIX, 16). Nevertheless, it bears mentioning that it is not uncommon to find in Biblical Hebrew of the MT consonantal text and Tiberian vocalization tradition an apparent *yiqtol* verb followed by a *wayyiqtol*, especially in poetry (e.g., Ps 44:3; 52:9; 55:6; 107:18).

(1QH^a XII, 21); ותוצא “and you will bring forth” (1QH^a XII, 26); יהי “and it will be . . .” (1QH^a XVI, 9); וימח “and he will wipe out . . .” (4Q215a 1 ii, 3); ותהי . . . ותהי “and there will be (trembling) . . . and there will be (destruction) . . .” (4Q385b 1, 3); ויכן “and he will establish” (4Q525 2–3 ii, 4); ויעש “(the person who) will act (arrogantly) . . .” (11Q19 LVI, 8); ויעש “and he will do . . .” (11Q19 LIX, 16); ויארך “and he will lengthen” (11Q19 LIX, 21); ויהי “and it will be . . .” (11Q19 LXII, 4); ויקרב “and he will offer” (11Q20 I, 23).

In relation to II-*yodh* roots and *hiphil* forms, Qimron (p. 164) states that the only exception (among non-biblical scrolls) is ויהכין “he will establish” (1QS III, 9), though other exceptions can be found, like ויביא אל בבל “and he will bring (him) to Babylon” (4Q386 1 iii, 1, unless ויביא = *wayābē* not *wayābī*); ויצב “and he will set up” (4Q471 2, 8); ויקטיר [ר] “and he will burn” (11Q20 1, 22); ויקטיר [ב] “and he will offer” (11Q20 2, 7). Without attempting to be comprehensive, he lists (p. 164 n. 27) six exceptions among biblical scrolls (three of which, he feels, may not really reflect a regular *yiqtol* form due to a final *yodh-aleph* digraph [e.g., ויביא reflecting *wayābē* not *wayābī*]). Still, there are other examples not cited: ויעיר “and he will arouse” (1QIsa^a at Isa 10:26 ≈ וְעוֹרֵר in MT; twice at Isa 50:4 ≈ וְעִיר in MT); ויצין “and he will blossom” (1QIsa^a at Isa 27:6 ≈ וְצִין in MT); ויאזין “and he will give ear” (1QIsa^a at Isa 42:23 ≈ וְאֶזֶן in MT). Among III-*waw/yodh* roots, Qimron (p. 165) lists four exceptions, though others can also be found: ויהיה “and it will be” (4Q175 3); ויטה “and (who) will pervert” (11Q19 LI, 17); ויהיה “and it will be” (1QIsa^a at Isa 4:3, 5:5 [twice], 8:14 [spelled וְהוּא in the scroll] ≈ וְהָיָה in MT); ויעשה “and he will make” (1QIsa^a at Isa 40:19); וישתחוה “and he will bow down” (1QIsa^a at Isa 44:17 ≈ וְיִשְׁתַּחוּ in MT); וישה “and he will do” (1QIsa^a at Isa 48:14 ≈ וְיַעֲשֶׂה in MT).

In the end, the syntagm *w*+short-*yiqtol* is most commonly used in the DSS to communicate the past indicative (identical to the *wayyiqtol* construction we are familiar with) and to communicate what appears to be a future indicative sense. Rarely, it seems to be used to express a volitive sense. Of these three uses, only the *w*+short-*yiqtol* communicating a future indicative is unexpected based on the MT/Tiberian tradition. Nevertheless, there are many exceptions to this usage in the DSS. The use of the short-*yiqtol* without preceding *waw* to communicate the same future indicative sense suggests (together with everything else) that many scribes did not distinguish semantically between the regular (or long-) *yiqtol* and the short-*yiqtol* forms.

p. 163, §C 2.1.3.3. The reference to 1QH^a IX, 40 should instead refer to line 30.

p. 176–78, §C 2.1.7.1. See the comments to p. 109, §B 2.

pp. 182–85, §C 3.1.1. This section discusses internal passive conjugations, including the passive *qal*. In general, the presentation seems confused.

Qimron several times asserts (e.g., p. 185) that the passive *qal* was replaced by the *niphal* in Late Biblical Hebrew and in the Hebrew of the scrolls. It is not quite so simple, however. (50) The historic passive *qal* seems to have been replaced not only by the *niphal* (e.g., יִלָּקַח “it will be taken” 1QIsa^a at Isa 49:25 ≈ יִלָּקַח in the MT) but also by the *qal* passive participle (e.g., אֲסוּרָה “[from a] strung [bow]” 1QIsa^a at Isa 22:3 ≈ MT אֲסוּרָה); by *pual* participles (e.g., מְמֻרָט “polished” 1QIsa^a at Isa 18:2 and 7 ≈ מְמֻרָט in the MT); by impersonal constructions (עָבְדוּ “[which] was done [lit., they did = עָבְדוּ]” in 1QIsa^a at Isa 14:3 ≈ עָבַד in the MT); (51) by verbs in the active voice (תַּחֲלִי “can it be in labor?” 1QIsa^a at Isa 66:8 ≈ יִהְיֶה “be born” in the MT). And, in addition, it was absorbed into the *pual* and *hophal* conjugations so that a form that was once read as passive *qal* was in the Second Temple era most likely read as a *pual* or *hophal* (e.g., יִצָּר “[which] will be formed” 1QIsa^a and 4Q57 [4QIsa^c] at Isa 54:17 ≈ MT יִצָּר). (52) By analogy or in imitation of the biblical idiom, scribes and writers continued to use these historic passive *qal* verb forms in later compositions, though they would have conceived of them as *niphals*, *puals*, *hophals*, etc. All of this is found in Late Biblical Hebrew texts, in Ben Sira manuscripts, and in the DSS. This evidence lets us assert without hesitation that the passive *qal* was not recognized as a conjugation or *binyan* in late Second Temple times.

The passive *qal* participle (distinct from the *qal* passive participle) is not described here or elsewhere in the grammar, though this form is distinct at least in the Tiberian Hebrew tradition and in the consonantal MT text (as reflected in the lack of a prefixed *mem*). Note the one clear example in the DSS: מוֹרָט “was made smooth” (4Q56 [4QIsa^b] at Isa 18:7 for MT מְמֻרָט).

Qimron lists (p. 183) numerous illustrations of finite forms of the passive *qal* (over seventeen examples), the *pual* (over twenty examples), and the *hophal* (over twenty-five examples). The presentation seems to imply that the three stems were attested relatively equally among the scrolls and continued to be used during this time. He goes on to claim

(50) See Eric D. Reymond, “The Passive *Qal* in the Hebrew of the Second Temple Period, Especially as Found in the Wisdom of Ben Sira,” in *Sibyls, Scriptures, and Scrolls: John Collins at Seventy*, ed. Joel Baden, Hindy Najman, Eibert Tigchelaar, 2 vols. (Leiden: Brill, 2016), 2:1110–27.

(51) See Kutscher, *Isaiah*, 401–402. Note, similarly, עָבְדוּ (in 4Q59 [4QIsa^c] at Isa 14:3).

(52) See Reymond, “Passive *Qal*,” 1115–17.

(p. 185) that while the passive *qal* was replaced by the *niphal* already in Late Biblical Hebrew, the *pual* was sometimes also replaced by the *niphal* in the DSS and sometimes replaced by the *hithpael*. And, in other cases, one of these internal passives was replaced by an impersonal construction. Yet, he writes (p. 185): “Even though the internal passive is rare, it is productive in this period.” In general, he seems to be saying that in the DSS the three internal passive stems (including the passive *qal*) continued to be used, but not as much as they once were. (53)

As has just been explained, it seems highly unlikely that the passive *qal* was still perceived as the internal passive stem to the *qal* in late Second Temple times. Qimron is right, however, that the *pual* and *hophal* were still productive. It seems inconsistent, therefore, that *pual* and *hophal* finite forms are considered in this section, apart from their participles, which are instead listed with *piel* and *hiphil* forms. All the *pual* forms should be discussed together; all the *hophal* forms should be discussed together. In the case of the *pual* participle, Qimron even writes (p. 237, §C 3.4.7.1):

These forms should not be considered as *pu'al*, which is diminishing in DSS Hebrew (see C 3.1.1). They should rather be taken as perfective passive participles of *pi'el* or *hitpa'el*.

Given the numerous *pual* finite forms cited and the recognized productivity of this stem, the above statement does not make sense. Although it is not stated explicitly, one suspects that it is inspired by the relative frequency of the forms in later literature. In the Mishnah, for instance, the *pual* is almost exclusively attested as a participle. (54) In the DSS, while there are more participles than finite forms, there are plenty of the latter group, implying (as Qimron himself recognizes) the productivity of this stem. (55)

p. 203, §C 3.2.3.9. In discussing the *yiqtol* conjugation of geminate verbs in the *qal*, Qimron implies by his presentation that those verbs where we expect to see an *o* or *u* theme vowel (based on historical evidence) usually have a *waw* mater after the first root consonant. He lists only one case where this does not happen, though there are many more: יזמו “they intend” in 4Q381 45a+b, 2 (cf. יזמו “they intend” in

(53) Note the sentence (p. 185): “This observation, while accounting for the reduction of the passive *qal* and *pu'al*, fails to account for the reduction of the *hup'al*.” The passive *qal* had not reduced; it had disappeared.

(54) Accordance lists 724 instances of the *pual* participle, but just a handful of perfect and imperfect forms.

(55) Accordance counts ninety-seven examples of the *pual* participles versus fifty-six finite forms among its corpus of non-biblical scrolls.

4Q171 1–2 ii, 14); יִשְׁמְרוּ “they will (not) destroy him” in 4Q174 1–2 i, 5; יִחְגּוּ “they will stagger” in 4Q418b 1, 4; the initial spelling of יִחְגּוּ “they will stagger” 4Q88 at Ps 107:27; וִישְׁלֹכָה “they will despoil you” in 1QpHab 8:15, 9:3 (in a quotation of Hab 2:8); יַעַר “let him (not) strip” in 4Q421 13, 5; (perhaps יִסְבּוּ “[th]ey will go around” in 11Q17 VII, 7). In these examples, note that many of the verbs are 3mp and the verbal stem is followed by a *waw* mater. Presumably, this reflects a tendency, reflected in other forms, not to include two maters in a single word (as with inflected feminine plural nouns and adjectives with a preceding *o* or *u* vowel, like לְדוֹרָתָם “for their generations” 11Q19 VIII, 13 [see p. 61, §A 2.1.2 and Qimron’s general comments on p. 63, §A 2.2]).

p. 208, §C 3.2.4.7. In discussing imperatival geminate verbs in the *qal*, Qimron does not cite the plene spelling of וַחֲתוּ “and be dismayed!” in 1QIsa^a at Isa 8:9. This agrees with the Tiberian vocalization, though the vowel is not indicated with a mater in the consonantal text of MT (i.e., it reads וַחֲתוּ). One may contrast the apparent *a* theme vowel in the *qal yiqtol* conjugation forms in the scrolls (p. 203) and in the MT. This seems to suggest a variation in the DSS as well as the MT.

p. 224, §C 3.3.3.1. The verb יִבְדִּילוּ is not found at 1QS IX, 6, but rather in line 5.

pp. 265–69, §§D 2.0–2. The table (on p. 265) and following discussion does not describe sufficiently the suffixes as they occur on plural nouns and on certain prepositions. This leaves many questions unanswered. In the table we might expect, for example, a transliteration of the 1cs suffix on plural nouns (i.e., *-ay* or *-ē* ?), 2fs (i.e., *-aykī* or *-ēkī* ?), 3mp (i.e., *-ēyyemma* ?), etc.

As for the articulation of the 1cs pronominal suffix on plural nouns and certain prepositions like עַל, one assumes that the earlier *-ayya* had contracted to *-ay* in the pre-Exilic era (as reflected, e.g., in the Tiberian vocalization tradition) but one wonders if this had further contracted to *-ē* in the dialect reflected in the DSS, based on the assumption of unstressed diphthong reduction (and presuming also a penultimate accent as with לְמַכְבְּדִיכָה “to the one honoring you” 4Q416 2 iii, 10 and בְּעוֹבְדִיכָה “when you abandoned” 4Q460 9 i, 8 [see pp. 68–69, §A 3.2.1 and pp. 144–46, §B 14.3]; cf. the apparent unstressed word-final diphthong reduction in the case of the 3ms suffix on plural nouns, i.e., *-aw* > *-ō* [pp. 130–31, §B 8.4.4])? Qimron seems to allude to this possibility in the section on phonology, when he cryptically writes (p. 130, §B 8.4.3): “The form חַיָּא ‘my life’ 4QSepHayob^s 25:9 [= 4Q222 1, 1] may also indicate that the diphthong *ay* had been contracted,” though he also notes that the word seems to have been corrected to חַיִּי. In other words, the spelling חַיָּא presupposes (if I understand correctly): ḥayyē.

Qimron writes (p. 267, §D 2.2.1.2) that since forms like דברִיכָה represent both singular and plural nouns there was no distinction in DSS Hebrew between the suffixes. That is, the suffixes were pronounced in the same manner *-eka* (with stress on the penultimate *e* vowel). But, a similar graphic form need not imply a similar pronunciation. Given the penultimate accent in such forms, should we not assume the preservation of the historical diphthong in the plural nouns, namely *-ayka* or *-aykā*? (56) Similarly, prepositions like עַל would exhibit a historical diphthong (*-ay-*): *‘ālaykā*. This would then imply an acoustic distinction between singular and plural nouns with the 2ms suffix, despite their similar graphic representation: דברִיכָה *dābārekā* “your word” vs. דברִיכָה *dābāraykā* “your words.”

For the 2fs suffix on plural nouns and prepositions like עַל, the historical diphthong would also have been preserved with penultimate stress: עליִכִּי *‘ālaykī*. By extension, one assumes that the 3fs suffix on plural nouns and prepositions might have been עליֵה *‘ālayyā* (< *‘ālayhā*) and the 1cp suffix on plural nouns and prepositions עליֵנו *‘ālaynū*. These too would have provided acoustic distinctions between singular and plural nouns (e.g., דברֵנו *dābārenū* “our word” vs. דברֵינו *dābāraynū* “our words”).

Such a preservation of the historical diphthong might prove unlikely if there were numerous examples of plural nouns (or prepositions like עַל) not followed by *yodh*. However, these seem particularly rare (at least from a preliminary survey). Among the non-biblical scrolls, Accordance lists over 800 examples with only four clear exceptions: מלחמותֶכָה “your wars” 1Q36 8, 2; ואורֶךְ “and your Urim” (4Q175 14); ממשלותֶךָ “your dominions” (4Q369 3, 4); צפורֶנָּה “her nails” (11Q19 LXIII, 12).

p. 272, §D 2.3.4. See comments to p. 131, §B 8.4.5.

p. 293 n. 17, §E 1.1.2. The relevant passage from 4Q76 at Mal 2:11 is given as בית צִיִּן but this is incorrect. It is, instead, בית אל נֹכַר. The phrase בת צִיִּן “daughter of Zion” is, to my knowledge, never spelled בית צִיִּן among the scrolls.

p. 305 n. 120, §E 1.2.3.2. The examples of variants (to nouns of the base *qitāl* and *qutāl*) in the last paragraph of the footnote are listed without reference to their sources; nor are cross references supplied to where they are discussed in the grammar. The prothetic *aleph* on שאֵל “Sheol,” אֵשׂאֵל, is found in 11Q5 at Ps 141:7, and is discussed on p. 142, §B 13. On the other hand, the sequence of examples תֵּאוּמִים,

(56) This assumes preservation of diphthongs in stressed syllables, which seems to be largely taken for granted, since Qimron notes in the section dealing with the diphthong *ay* (p. 130, §B 8.4.3) no evidence to the contrary.

תמים and תמים are unknown to me and I could not find them listed in the word index. I am not sure what word among the scrolls they are meant to be variations of. A word with the form והתמים occurs twice (in 4Q164 1, 5 and 4Q174 6–7, 7) though this is commonly understood as the adjective תמים “perfect” with the *a* vowel shifting to *u* before the *mem*, as elsewhere (a phenomenon discussed on p. 138, §B 11, though תמים is not listed there as an example).

pp. 324–25, §E 2.2.0. In reviewing the various bases for verbal-nouns associated with the verbal stems or *binyanim* (e.g., *qal* is associated with *qutul*, *maqtal*, *qitil*), Qimron writes: “There are no respective verbal-nouns of the passive and reflexive verb conjugations. The passive voice . . . is unmarked in verbal-nouns.” It is hard to reconcile these statements with the illustrations on p. 331 §E 2.4 of the מקטל pattern, i.e., the *pual* participle. In any case, the examples of the מקטל pattern should not be included in the description of verbal nouns; these are simply participles, as implied by the productiveness of other *pual* forms (see the above comments on pp. 182–85 §C 3.1.1).

p. 352, §E 4.1. In the sub-section c.3 (which lists gentilic plural nouns with one *yodh*), the partially reconstructed form [הלויים] from 4Q471 1, 5 appears. At first, one might think this is a typo in the grammar or an example which is misplaced and which belongs with those nouns that exhibit two *yodhs*. However, the digital photos (at <https://www.deadseascrolls.org.il>) suggest in fact that the reading should be instead [ה]לויים. (57) The *lamedh* is clear based on the traces: the tip of its ascender appears above the lacuna; the tip of its left horizontal bar appears next to the following letter; and the bottom tip of its angled lower line is also clear. The following letter must be a *waw*, based on its relative size and its smaller head (as compared to the following letter, which is clearly a *yodh*).

p. 354, §E 6. The section details various nouns “not mentioned in the above discussions,” though, in fact, many have been discussed. For example, כאוב is discussed on p. 324, §E 2.1.3; משה and משה are listed on p. 304, §E 1.2.2.6, the former being described in a footnote (n. 113), and with comparative evidence cited; מבנית is listed as a *maqtil* noun on p. 315, §E 1.4.2; the plural form נגועים is mentioned in a footnote to נוגע from 1QIsa^a at Isa 53:8 on p. 320 n. 205, §E 2.1.1.

p. 357, §F. With the pair of roots יעף and עף should be mentioned the alternative עף that appears frequently in the biblical scrolls (e.g., in the passage cited from 11Q5 at Ps 143:6 where יעף is corrected to עף). With regard the pair ישוב and שוב, no examples are quoted, just

(57) Qimron’s reading in his edition (*The Dead Sea Scrolls: The Hebrew Writings*, 3:210) is also incorrect.

the passages. In these cases, it seems possible that שׁוּב simply means “return.”

p. 367, §G 2.4. According to an Accordance search, further examples of the adverbial *heh* include צפונה “to the north” in 4Q364 17, 5 and 11Q20 X, 6; ימה “to the sea” in 4Q521 1 ii, 4.

p. 368, §G 2.5 and p. 405, §H 2.2. Both sections address the adverbial (or directive) *heh* on place names and then present examples. On p. 368, three examples are introduced with the words “In the DSS we find the following cases,” from which one might infer these were the only cases, but on p. 405 another six examples are listed. On p. 405 there is a cross-reference to “adverbs terminating in *a*,” but not to place names ending in *a*. All the examples should be listed and discussed together.

p. 369–70, §H 1.1. The *wayyiqtol* construction of classical Biblical Hebrew is described as composed of a *waw* “prefixed to . . . the indicative imperfect.” More precisely, the *wayyiqtol* is made up of a *waw* followed by the short-*yiqtol* form that is often homophonous and homographic with the regular (long-)*yiqtol*.

On p. 370, only a short paragraph tangentially references *wāqṭaltī* forms (i.e., the *waw*-consecutive perfect). The significance of this form’s use with the regular *wā* + regular (long-)*yiqtol* forms certainly deserves more comment.

pp. 371–73, §H 1.2. Qimron argues that the “distinctive” and “peculiar” forms of the *yiqtol* conjugation (i.e., the short-*yiqtol* forms [= the shortened and apocopated forms of the 3ms, 3fs, 2ms in II-*waw/yodh*, III-*waw/yodh* roots and in the *hiphil*] as well as the cohortative) are typically used in the initial position of a clause. He goes on to write: “Moreover, even elsewhere, when the verb comes at the beginning of the sentence, only the peculiar forms are used” (p. 371). Nevertheless, the examples cited for the short-*yiqtol* forms do not support his thesis. Out of the four examples of the short-*yiqtol* from non-biblical scrolls that appear at the beginning of a clause, three may really not belong in this specific category. Two words (יַעַר [“he will arouse” 4Q176a 1–2, 3 at Jub 23:23 and יָשַׁם [“he will set” 1Q28b V, 26) are conventionally reconstructed with a preceding *waw* conjunction and therefore would follow the general pattern of *waw* conjunction + (future indicative) short-*yiqtol* and thus belong in the lists on pp. 163–64, §C 2.1.3.3. In addition, the verb יִשְׁלַךְ “he will throw” (4Q458 1, 8) may instead be read וְשָׁלַךְ “and he threw” or “and he will throw.” Furthermore, Qimron notes at least three clear counter-examples (where the short-*yiqtol* form, representing the future indicative mood, does not appear at the beginning of a sentence or clause, noted on pp. 371–72 n. 7). Other cases not cited can also be found: כִּי־אֵלֶּם אֵל . . . וְאַרְץ תִּצְרַח “and

the earth will cry out [presumably *hiphil*] . . . for God will thunder [also *hiphil*] . . .” (1QH^a XI, 34–35); וְכֵן יֵבֶן “and thus he will understand . . .” (4Q266 9 iii, 5) and וְרַעֲבִים יַעֲשֶׂה “and the hungry he will enrich” (4Q521 2 ii + 4, 13). Such a distribution does not really inspire confidence in this thesis as it relates to third and second person forms.

The numerous examples of *waw* conjunction + (future indicative) short-*yiqtol* (listed pp. 163–64, §C 2.1.3.3 [see comments above]), therefore, should not be understood as exemplifying the rule that a short-*yiqtol* form must occur in clause initial position. Instead, it seems simply that after a *waw* conjunction (whether a simple *waw* or a *waw* that is part of a *wayyiqtol* expression) a short-*yiqtol* form or cohortative is expected. This implies, in turn, that the distinctive volitive sense of *waw*+short-*yiqtol* had been lost and that the form could indicate either a future indicative or a volitive sense. In addition, it might be added that the examples *waw* + (future indicative) short-*yiqtol* do not seem to imply a purpose-result nuance: וּמִשַּׁחַת יִחְלֹץ נַפְשִׁי וַיִּכֵּן לְדֶרֶךְ פַּעֲמֵי “he will withdraw my soul from the pit and steady my steps on the path” (1QS XI, 13).

p. 388, §H 1.6.4. Paragraph 2 to this section claims to describe the syntagm לֹא + infinitive, yet of the seven examples, three express the syntagm אֵין + infinitive. These should be listed in paragraph 3 of this section, on p. 389.

In footnote 55 a comparison is drawn between the construction אֵל + infinitive in 4Q392–393 13, 3 and 5 and the phrase אֵל תֹּאמַר in Ben Sira 39:46. The phrase אֵל תֹּאמַר does not occur in chapter 39 of Ben Sira and, of course, is not composed of an infinitive expression. Intended is the phrase אֵל לֹאמַר in Sir 39:34 (where אֵין is found in the margin).

p. 405, §H 2.2. See comment to p. 368, §G 2.5.

p. 429 n. 182, §H 4.8. Reference is made to the expression זֶה מִזֶּה in Sir 42:24. This phrase occurs only in the medieval manuscript B. It would have been more useful to note the parallels to this syntagm in the Masada scroll, namely the partially reconstructed זֶה לַעֲמֵת זֶה [זה] “[this] beside that” (Sir 42:24) as well as the expression זֶה עַל זֶה “this above that” which occurs in the very next verse (Sir 42:25).

pp. 447–519. The word index alphabetizes Hebrew words according to their inflected forms (including with prefixed prepositions), according to their spelling with maters, and according to their idiosyncratic spelling in the scrolls. So, for example, to find all the instances in which the verb מָלֵא “to fill” appears in the grammar requires one to look up the verb itself, sub מָלֵא, then under its *qatal* and *yiqtol* conjugation forms (e.g., יָמַלֵא), then according to its idiosyncratic

spellings with final *heh* (e.g., מִלָּה), then with prefixed prepositions (e.g., בַּמִּלָּה and לַמִּלָּה), etc. Nevertheless, some forms are cited with prepositions in the text, but not alphabetized according to the preposition in the index (e.g., מִרְפָּה [ל] “for healing” is cited in §A.5.1, but listed in the index under מִרְפָּה; similarly, בְּלִדְתָּהּ “in their birthing” is cited in §D.2.6.3 but listed in the index under לִדְתָּהּ). Whatever the advantages of this alphabetization scheme are, the disadvantages certainly outweigh them.

Conclusion

Qimron’s new grammar of the Hebrew of the scrolls represents a real advancement in the study of the language of these manuscripts. Nevertheless, there are far too many errors in it. These make the reading and comprehension of the grammar more difficult than it should be and sometimes undermine the analysis itself. In addition, the neglect or deemphasis of certain linguistic phenomena limits the usefulness of this tool. Although it is a rich resource of information on the language of the scrolls, it should be used with caution.

APPENDIX

Mistakes in cross-references are extremely frequent and it makes finding references difficult. I have not done a comprehensive search of these, but instead list just those I have encountered more or less randomly:

- p. 60 n. 10. The reference C.3.2.6.4 should be instead C.3.2.6.5
- p. 61, final line of first paragraph, B.9.2 should be instead B.10.2
- p. 61, final line of second paragraph, A 2.2 above should be A 2.2 below.
- p. 62, final paragraph, three lines up from bottom, B.1.2.4 does not exist; intended is B.1.2.1.3
- p. 82, second paragraph, second line from bottom, the reference to note 67 should be to note 68
- p. 96, first paragraph, fifth line, B 8.6 should be B 8.4.1 — B 8.6 does not even exist!!
- p. 96, first paragraph, second-to-last line, B 8.3 should be B 8.2.1
- p. 100, second paragraph, four lines up from bottom, B.11.3 does not exist and should be B 1.1.3.
- p. 102, second paragraph, six lines down, B 1.1.1 should be instead B 1.1.1.1.
- p. 105, second paragraph, last line B 9.1 should be instead B 9.7, 9.10
- p. 124, second paragraph, third line B 7.2.4–B 7.2.7 should be instead B 8.2.5
- pp. 131–32, B 8.5, 8.6, and 8.7 do not even exist!!
- p. 132, first paragraph, three and four lines down, B 8.6 should be B 8.4.1 and B 8.7 should be B 8.4.2

- p. 136, first paragraph, last line C 3.2.3 should be instead C 3.2.3, 3.2.4, and 3.2.5
- p. 141, second paragraph, three lines down, “cf. note 18 in A 3.1 and A 3.2-end” should be “cf. note 27 in A 3.1 and note 38 in A 3.2”
- p. 152, first paragraph, third line, B 8.8 should be instead 8.4.5
- p. 152, second paragraph, last line, B 8.6 should be instead 8.4.4
- p. 155 second paragraph, last line B.11 should be instead B.12
- p. 224, last line, B 14.3.1 should be B 14.3
- p. 315 n. 178, last line, E 2.11 should be E 2.10
- p. 315 n. 180, last line, “note 173” should be “note 172”
- p. 319 n. 196, E 2.14 should be E 2.12
- p. 396 n. 76, the reference to “note 74” should be to “note 72”

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EXERCICES DE DEUX SCRIBES À *KHIRBET* QUMRÂN : *KhQ* 161 ET *KhQ* 2207

Sommaire

Cette note reprend l'étude des deux exercices de scribe trouvés à *Khirbet* Qumrân. *KhQ* 161 est un témoin unique de la présentation de la séquence des lettres commençant par la deuxième moitié des 22 lettres avec la signature du scribe. Les restes de *KhQ* 2207 attestent un exercice de scribe copiant des phrases ou sentences de sujets fort divers. L'un et l'autre exercice témoignent de la présence de scribes inspirés dans l'apprentissage de leur art.

Summary

This note takes up again the study of scribal exercises found at *Khirbet* Qumran. *KhQ* 161 testifies to a unique sequence of letters beginning with the second half of the 22 letters with the scribe's signature. The remains of *KhQ* 2207 shows a scribe's writing exercise with sentences of quite different matters. Both of them attest the presence of scribes experimenting with their art.

PARMI les objets inscrits retrouvés sur le site de *Khirbet* Qumrân figurent un fragment de jarre inscrit à l'encre noire et une pierre de calcaire portant des lettres à l'encre noire. Le fragment de jarre *KhQ* 161 provient d'une trouvaille dans la Tranchée A, "à un mètre de profondeur", enregistré le 20/02/53, (1) et la pierre *KhQ* 2207 est en

(1) Voir R. de Vaux, *Fouilles de Khirbet Qumrân et de Ain Feshkha. I - Album de photographies, Répertoire du fonds photographique, Synthèse des notes de chantier*, présentées par J.-B. Humbert et A. Chambon, NTOA.SA 1 (Fribourg - Göttingen : Éditions Universitaires - Vandenhoeck & Ruprecht, 1994), p. 339b, le 17/02/53 : « À 35 m au nord du grand bâtiment, en bordure du wadi, dans une couche non stratifiée,

fait une trouvaille de "surface" d'après la fiche d'enregistrement ; elle provient du *locus* 129 situé "dans l'axe nord de la citerne 118, en bordure extérieure du bâtiment", trouvée le 26/02/55. (2) Ce jour-là "apparaît un mur de séparation nord-sud, la partie orientale devint le *locus* 133". Le nouveau *locus* 172 (= *loci* 129+133) a été interprété dernièrement comme une porterie ou une écurie à cause d'un pavement et d'un drain d'évacuation des eaux usées. (3)

1 – L'ostracon hébreu *KhQ* 161 : un élémentaire, voir photographie PAM 40.405 et figure 1.

Un tesson de jarre blanchâtre, de 13,5 × 11 × 0,4 cm, de forme grossièrement triangulaire (il manque un petit éclat triangulaire au milieu du côté droit) porte des lettres sur le côté convexe. Les lignes sont numérotées ici selon l'ordre d'écriture et non de haut en bas comme c'est généralement le cas, (4) à l'exception du fouilleur (5) :

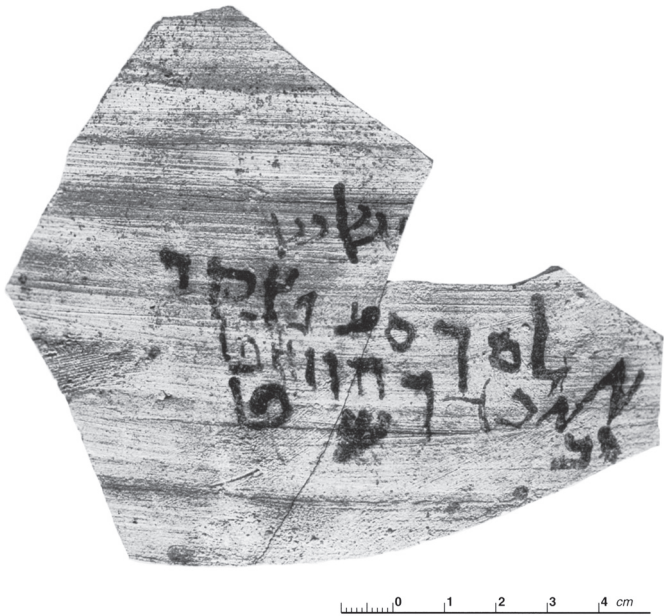
les bédouins avaient commencé un trou. Milik en sort des tessons et les fragments d'une assiette et d'un bol. ... **161** (à un mètre de profondeur) : ostracon en hébreu » ; R. de Vaux, « Fouilles au Khirbet Qumrân. Rapport préliminaire sur la deuxième campagne », *RB* 62 (1954) 206–236, p. 229, écrit : « Un tesson plus grand, trouvé dans la tranchée A et probablement attribuable à la période I, portait un alphabet. » ; R. de Vaux, *Archaeology and the Dead Sea Scrolls*, The Schweich Lectures of the British Academy (London: Oxford University Press, 1973), p. 103: «... in particular a sherd bearing a complete alphabet, the exercise of a pupil-scribe.»

(2) Voir R. de Vaux, « Fouilles de Khirbet Qumrân. Rapport préliminaire sur les 3^e, 4^e et 5^e campagnes », *RB* 63 (1956) 533–577, p. 565 : « Sur une autre plaque de calcaire, ébréchée de tous les côtés, cinq lignes sont inscrites en noir, mais on n'ose encore proposer aucune lecture pour ce texte très effacé », et R. de Vaux, *Fouilles de Khirbet Qumrân et de 'Ain Feshkha*, op. cit., p. 332b, « **2207** : plaque de calcaire inscrite ».

(3) Voir J.-B. Humbert, A. Chambon et J. Młynarczyk, *Khirbet Qumrân et 'Ain Feshkha. Fouilles du P. Roland de Vaux. IIIA - L'archéologie de Qumrân, Reconsidération de l'interprétation, Corpus of the Lamps*, NTOA.SA 5a (Fribourg - Göttingen : Éditions Universitaires - Vandenhoeck & Ruprecht, 2016), p. 438–439.

(4) Voir M.D. Coogan, « Alphabets and Elements », *BASOR* 216 (1974) 61–63, p. 61 ; E. Eshel, « 3. Khirbet Qumrân Ostrakon », in *Qumrân Cave 4 XXXVI. Cryptic Texts and Miscellanea, Part 1*, DJD XXXVI (Oxford: Clarendon Press, 2000), p. 509–512, avec une excellente photographie. La publication des objets épigraphiques qui m'avait été confiée en 1986, a été faite par A. Lemaire, « Inscriptions du Khirbeh, des grottes et de 'Ain Feshkha », in *Khirbet Qumrân et 'Ain Feshkha. II - Études d'anthropologie, de physique et de chimie/Studies of Anthropology Physics and Chemistry*, présentées par J.-B. Humbert OP et J. Gunneweg, NTOA.SA 3 (Fribourg - Göttingen : Academic Press - Vandenhoeck & Ruprecht, 2003), 341–388, p. 341–342, mais elle est à revoir en plusieurs endroits.

(5) De Vaux, cit. *RB* 1954, p. 229 : « Il commence en bas à droite ; en passant à la ligne suivante, le scribe a omis *yod* et *caph* ; les lettres *shin* et *taw* se trouvaient dans le petit fragment qui manque au bord supérieur et la trace de *taw* est visible à la cassure. Plusieurs lettres ont ensuite été reprises autour de ces lignes d'écriture. On notera la maladresse de la graphie : c'est l'exercice d'un scribe débutant. »



Photographie de l'ostracon *KhQ* 161, PAM 40.405

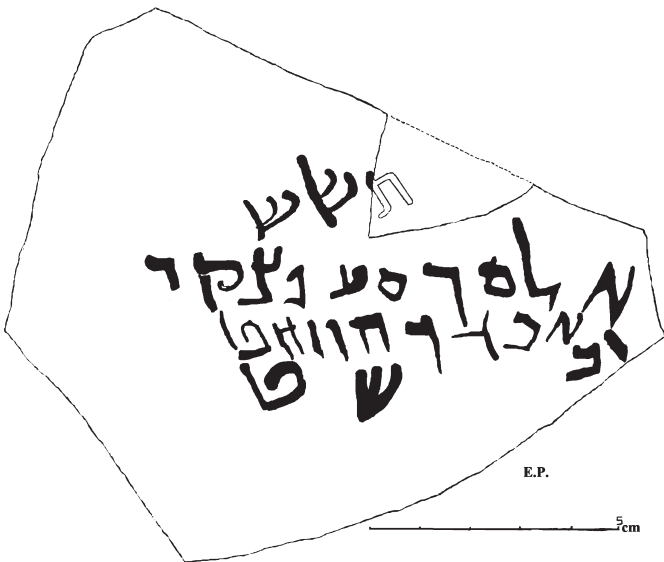


Figure 1 : Ostracon *KhQ* 161 = Élémentaire

תש	2
למןסעפצקר	1
אאבגדהווחט	3
יכ ש ט	4

– Ligne 1 : L'apprenti scribe a commencé à écrire la deuxième moitié de l'alphabet, (6) de *lamed* à *reš*, assez gauchement en ne retenant que les formes de *mem* final au module réduit et de *nun* final gauche, mais poursuit avec *pe* et un grand *šade* médians, non les formes finales, (7) ne retenant ainsi qu'une forme pour chaque lettre. Le scribe a délibérément choisi de finir sa ligne avec *reš*, soit 9 des 11 lettres, et à continué par un retour au-dessus.

– Ligne 2 : De cette ligne tracée de gauche à droite (?), il a écrit deux fois *šin* (soit pour remplir l'espace ou pour *šin* et *šin* mais qui ne comptent alors que pour une lettre dans les $9+2 = 11$ lettres de *lamed* à *taw*), puis du *taw* il reste encore le départ du jambage gauche. (8)

– Ligne 3 : L'apprenti scribe a commencé la ligne sous le pied du *lamed* avec un '*alef* tracé en forme de N couché à droite dont le jambage final dépasse à droite. La tête de *bet* est réduite ainsi que son jambage avec une longue base au tracé concave, ductus gauche. *Dalet* n'a pas de tête marquée mais une longue hampe tel un *kaf* final. La ligne s'arrête délibérément avec *tet*, soit encore 9 lettres comme à la ligne 1.

– Ligne 4 : Il écrit les deux autres lettres, *yod* et *kaf* médian ramassés à cause du bord inférieur du tesson. S'apercevant qu'il n'a pas respecté l'alignement à droite avec la ligne 3, il ajoute un grand '*alef* au-dessus, un peu dans l'interligne de 1 et 3, ainsi l'alignement marginal de 3 et 4 est alors respecté.

Après l'écriture d'un 'élémentaire', gauchement tracé mais assez réussi tout de même dans sa disposition symétrique sur 4 lignes, le scribe a ajouté un *šin* sous le *he* et plus loin un *tet*, qui, avec le *reš* dépassant à gauche à la ligne 1, composent sa signature, à lire שטר "scribe, secrétaire". Ce témoignage d'écriture est celui d'un apprenti scribe, qui a appris aussi cette séquence et non seulement la plus courante, un abécédaire de '*alef* à *taw*. Dans ce cas on s'explique mieux les ajouts de

(6) De Vaux, *cit.* RB 1954, p. 229, fait commencer l'écriture à la ligne 3, puis passe à la ligne 1 suivie de la ligne 2 de notre numérotation. Eshel, *cit.*, p. 510, retient l'ordre suivant : lignes 3, puis 4, puis 1 et enfin 2. De même Lemaire, *cit.*, p. 341, avec « un double Š, peut-être suivi d'un T à droite, selon la proposition de R. de Vaux. Enfin, il semble avoir tenté un nouvel essai de Š au milieu de la ligne 4 ». Mais de Vaux a lu la ligne 2 dans le sens inverse.

(7) Des apprentis scribes n'hésitent pas à écrire les deux formes des lettres médianes et finales dans leurs alphabets, voir par exemple *Murabba'ât* 79 (il y manque le *mem* médian), *Mur* 80, *Mur* 10B.

(8) Eshel, *cit.*, p. 510, estime que manque la fin de l'alphabet dans la lacune (d'une écriture de droite à gauche), et que le scribe a ajouté deux autres *šin*, (c'est aussi possible).

šin et *tet*, à la ligne 4, comme pendants des deux *šin* à la ligne 2. Il n'est pas à proprement parler un scribe sans formation, mais il a déjà acquis une certaine instruction.

L'intérêt de cet ostracon est d'être un des rares exemples de l'écriture des deux moitiés égales de l'alphabet, $11 + 11 = 22$ lettres, qu'on peut apprendre et écrire aussi bien de *lamed* à *taw* que de *'alef* à *kaf*. Dans ce cas, on ne parle plus d'"abécédaire" pour la séquence a-b-c-d ou d'alphabet/*alphabeton* pour *'aleph-bet*, mais d'"élémentaire", puisque la séquence de la deuxième moitié dans le sémitique du nord-ouest est toujours *el-em-en* finissant par *taw*, soit les *elementa*, (9) dont le mot désigne "les lettres de l'alphabet" (Suétone, *Vies des Césars*, 56). On ne peut pas invoquer le manque d'espace à la fin des lignes 1 et 3 pour écrire la suite, ni la proximité d'une anse, hors propos sur cet ostracon pour expliquer l'ordre de l'écriture. (10) Mais la disposition du modèle d'écriture, quoique un peu gauche dans les tracés, est tout à fait recherchée, et non celle d'un apprenti étourdi, ajoutant des lettres n'importe où et n'importe comment. Tout paraît soigneusement recherché.

La graphie tout comme le lieu de découverte "à 1 m de profondeur" dans la tranchée A bien plus profonde, daterait cette écriture vers le milieu du 1^{er} siècle avant J.-C. (voir figure 3), soit la fin de l'époque hasmonéenne, la période Ib de de Vaux. (11)

Une telle séquence *lamed-taw* (mais avec séquence *pe-'aïn*) puis *'alef-kaf* serait peut-être à reconnaître dans les Psaumes 9 et 10 (= Psaume 9 LXX), en remettant le Psaume 10 (ת-ל) avant le Psaume 9 (ז-ח) pour retrouver dans l'unique «Psaume 9» la séquence habituelle de la complainte suivie de l'action de grâces pour la délivrance. (12) L'ordre du texte psalmique présentement difficilement compréhensible pourrait être dû à une hyper-correction d'un copiste voulant retrouver l'ordre alphabétique habituel d'une composition acrostiche, en découpant pour cela le texte en deux Psaumes : 9 avec titre et 10 sans titre (TM, Syriaque), alors que le grec ne connaît qu'une seule composition (Psaume 9) mais dans un ordre inhabituel. Des corruptions de quelques

(9) Dénomination *elementum* déjà suggérée par Coogan, *cit.*, p. 62b.

(10) Eshel, *cit.*, p. 509–510, même écrit sur une jarre, la proximité de l'anse ne peut être invoquée, à plus forte raison sur l'ostracon. Pour la division de l'alphabet en deux moitiés, on ne peut invoquer les exemples de l'Hérodition ni d'Éléphantine, où la division est due à l'espace sur la surface disponible, ce qui n'est pas le cas sur cet ostracon.

(11) Voir A. Yardeni, *Textbook of Aramaic, Hebrew and Nabataean Documentary Texts from the Judaean Desert and Related Material*. B Translation . *Palaeography . Concordance* (Jerusalem: The Ben-Zion Dinur Center for Research in Jewish History, 2000). Les ductus des lettres sont tous connus autour du milieu du 1^{er} s. av. J.-C.

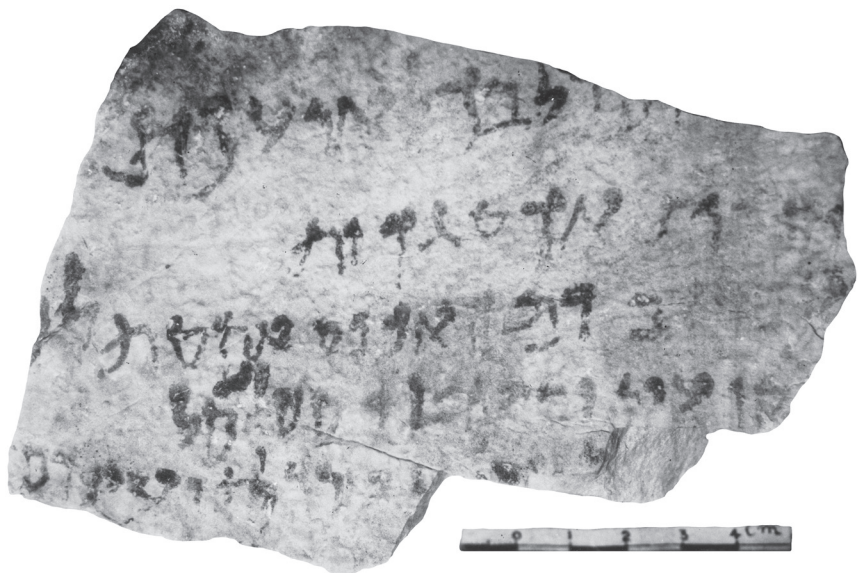
(12) Voir H. Eshel and J. Strugnell, «Alphabetical Acrostics in Pre-Tannaitic Hebrew», *CBQ* 62 (2000) 441–458, p. 453–458.

versets autoriseraient à envisager un archétype assez ancien, antérieur à la traduction grecque. Quoi qu'il en soit, l'élémentaire *KhQ* 161 pourrait appuyer cette manière de comprendre les Psaumes 9–10 dans leur composition originelle.

2 – La pierre de calcaire inscrite *KhQ* 2207 : copie de quelques sentences en hébreu

L'objet en question est un fragment de plaque de calcaire jaunâtre, de 15,5 × 10,5 × 2,5 cm mais incomplète en toutes ses dimensions, portant des restes d'écriture à l'encre noire. (13) Il semble qu'on a

(13) Voir Lemaire, « Inscriptions du Khirbeh, des grottes et de 'Ain Feshkha », *cit.*, p. 360b-362. Lemaire ne signale que des restes de cinq lignes et écrit : « Le caractère fragmentaire, l'inégalité de la surface et l'irrégularité des formes des lettres rendent la lecture très difficile. R. de Vaux y avait provisoirement renoncé. À titre d'essai, nous proposons : ... Les formes irrégulières des lettres, et l'absence — ou quasi absence — de séparation entre les mots trahissent probablement un scripteur au début de l'apprentissage de l'écriture. ... Au total, il s'agit probablement d'un exercice d'apprentissage d'écriture incomplet, qui peut avoir été la copie d'un extrait de Psaume ou d'oracle prophétique invitant à la reconnaissance universelle de la divinité. D'après la paléographie, la datation ne peut être que très approximative : vers le tournant de notre ère. » Lemaire remercie P. Marin-Boucher qui a préparé le fac-similé de la p. 361. S. White Crawford, « The Inscriptional Evidence from Qumran and its Relationship to the Cave 4Q Documents », in *The Caves of Qumran. Proceedings of the International Conference, Lugano 2014*, ed. by M. Fidanzio, STDJ 118 (Leiden/Boston: Brill, 2017), 213–220, p. 216b : « KhQ2207, a stone plaque with five lines of writing, has an unknown content. That is, the letters are random, rarely forming any recognizable word. Further, the letters are roughly formed and irregular. Lemaire suggested that it was “probably an incompleated apprentice scribe's exercise” ». R. Donceel, *Synthèse des observations faites en fouillant les tombes des nécropoles de Khirbet Qumrân et des environs. The Khirbet Qumran Cemeteries. A Synthesis of the Archaeological Data, The Qumran Chronicle*, Vol. 10, Cracovie/Cracow, 2002, p. 69, écrit : « on ne peut exclure qu'une dalle de remploi utilisée dans la couverture d'un *loculus* ait présenté des traces d'écriture : en février 1955 on a en effet découvert à Khirbet Qumrân une plaquette de calcaire portant 5 lignes d'une inscription tracée à l'encre. (note 198) ... En cours de publication à Louvain-la-Neuve. » Mais il est exclu que le *loculus* ait été couvert de lauzes ! La mention est due à P. Donceel-Voûte, « Le mobilier en pierre de Khirbet Qoumrân et 'Ain Feshkha », in *Matériel archéologique de Khirbet Qoumrân et 'Ain Feshkha sur la Mer Morte, pierre, lampes, verre, matériaux divers*, par R. Donceel et P. Donceel-Voûte (Presses universitaires de Louvain, 2017), p. 81 (Pl. 48.2), où l'auteur signale « *locus* 129; niveau inférieur (*sic*). Chaque ligne conservée peut avoir porté une quinzaine de lettres, écrites d'une main sûre et régulière. La peinture étant très usée, seules quelques lettres, identifiées comme appartenant à de l'hébreu palestinien, ont pu être reconnues de manière sûre, mais elles ne permettent aucune lecture cohérente. » En note 139, sont données les lectures de J.-M. Auwers (*infra*), et une conclusion de l'auteur n'acceptant pas la conclusion d'un apprentissage d'écriture incomplet de Lemaire, « ce que nous ne pensons pas car les lettres sont tracées et quelques beaux jambages sans hésitation. ... mais les mots identifiés peuvent être d'une teneur tout à fait profane. », en réaction au caractère psalmique ou prophétique suggéré par Lemaire. Dernièrement H. Misgav, « The Ostraca », in *Back to Qumran. Final Report (1993–2004)*, by Y. Magen and Y. Peleg, Judea & Samaria Publications 18 (Jerusalem, 2018), 431–441,



Photographie de la pierre KhQ 2207, PAM 41.596

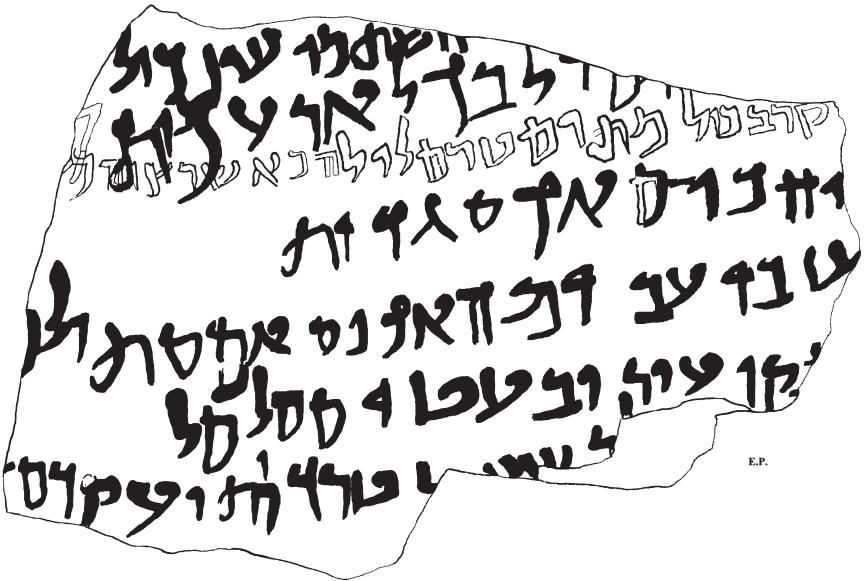


Figure 2 : KhQ 2207 : Exercice de scribe

affaire à une surface plane utilisée plusieurs fois pour apprendre à écrire, tant des lettres en partie effacées se retrouvent entre les lignes et sous les lignes du dernier usage, plus lisibles sur la photographie à l'infra-rouge PAM 41.596. Sur les photographies en couleur de 1994 ne sont plus visibles que de maigres traces. (14)

2.1. *Le texte, voir photographie PAM 41.596 et figure 2*

א/השתמו עין מל]	1
ולמד לבך לא רע מות]	2
ת]קבר כול מתים טרם לילה כאשר צוה מ]ושה	2'
ו]חכים אך סגדות	3
(י]עזבו עביו האך פס אם סתו למ]טר	4
צ]מקו עור ובעטו סס לסל	5
כ]לב שע[י]ר טרד וי> תוע קדם צ]אן	6

2.2. *Traduction*

- ¹ ...]ont bouché une source/la source de(?) tous[les ...
² ...]et enseigne à ton cœur, la mort n'est pas un mal ![
^{2'} ...]ensevelis chaque mort avant la nuit comme a ordonné M[oïse.
³ ...]et attendant uniquement des prosternations/vénération.
⁴ ... *quand*]les nuages disparaissent, est-ce assurément fini avec l'hiver pour la p[luie ?
⁵ *Le vent chaud et le soleil ri]*dent la peau et ils repoussent la vermine au panier.
⁶ ... *Un/Le ch]*ien a chassé un bouc et il erre au-devant du t[roupeau (de chèvres).

2.3. *Notes de lecture*

– Ligne 1 : À droite du deuxième *lamed* de la ligne 2, restes de *šin* précédé de jambages parallèles, *waw-yod/he*. À gauche du *lamed*, sont lisibles un *taw* bouclé suivi apparemment de *mem* et de *waw*, puis *'aïn*, *yod* probable et *nun* final, puis un grand *kaf*, *waw(?)* et *lamed*. Les restes de cette ligne n'ont pas été lus dans l'édition. Cette ligne ne paraît pas appartenir à une écriture antérieure et partiellement effacée.

– Ligne 2 : À la cassure, bas de jambage suivi d'un tracé oblique à gauche croisant légèrement le jambage droit d'un *mem* suivi de *dalet* ou *reš*, soit *waw-lamed* vraisemblables. Les mots suivants ne font aucune difficulté de lecture : *lamed-bet-kaf* final, puis *lamed-'alef*, puis

p. 431, écrit «Another sherd (KhQ 2207) contains numerous letters; it is extremely difficult to form any logical combination of words from them: ... Parts of words can be formed in different places, but the inscription as a whole is unintelligible. If this is not a writing exercise, it might have had some magical connotation.»

(14) Voir Lemaire, *cit.*, p. 360 : « EBAF 1994 : 16.911 », photographie p. 362, mais Lemaire n'a pas noté ce genre de traces, ni la photographie PAM 41.596.

reš-*'aîn*, enfin *mem-waw-taw* dont le trait coudé touche le *waw* et à la cassure reste probable de *kaf* final, lettre plus effacée ou délavée, tout comme le *lamed* de *l'*, mais doit appartenir à une écriture antérieure. (15)

– Ligne 2' : Entre les lignes 2 et 3, on peut encore lire des restes de lettres mal effacées : *qof-bet-reš*, puis *kaf-waw-lamed*, puis *mem-taw* bouclé, *yod-mem* final, puis *ṭet-reš-mem-lamed-yod* possible *-lamed-he*, puis *kaf*-*'alef-šin-reš* et apparemment *mem*.

– Ligne 3 : À la cassure, *yod* ou *waw*, puis *ḥet* difficilement *he*, *kaf* mais difficilement *bet* ou *dalet*, *waw* ou *yod*, *mem* final (sans espace entre les mots) difficilement *ṣade* ou *taw*. Puis la lecture est assurée : *'k sgdwt*. (16)

– Ligne 4 : La lecture paraît assurée : *'aîn* conservé en bonne partie, *zāin-bet-waw* à grande crosse, puis *'aîn-bet* et après un petit espace *waw* ou *yod* à grande crosse et *mem* médian, *he* un peu délavé, *'alef-kaf* final réduit, *pe-samek*, *'alef-mem* final, *samek-taw-waw* un peu délavé, *lamed* et partie de *mem* final. (17)

– Ligne 5 : À la cassure, légères traces de lettre correspondant au tracé de *mem* médian. Plus loin *waw-reš* suivi de *waw-bet*-*'aîn-ṭet* et *waw* à tête triangulaire. Puis lecture assurée. (18)

– Ligne 6 : Après une trace de départ de hampe de *lamed* possible à gauche du *reš* de la ligne 5, la partie de la tête de la lettre peut être celle de *dalet* ou *reš*, sans exclure *bet*, *mem* ou *nun* finaux, puis sont reconnaissables *šin* et *'aîn*, après la petite cassure tête de *dalet* ou *reš*, puis *ṭet-reš-dalet* certains, puis tracé d'un large *waw* suivi de *taw* surmontés de l'addition de *yod*, puis *waw* et un grand *'aîn*, enfin *qof-dalet-mem* final certains, et un départ de trait horizontal, sans doute *ṣade*. (19)

(15) Lemaire, *cit.*, p. 360–361, a lu : ?]---LBD L'W/Y/R'N/KT/H[?, et traduit « ?]... seul il ne répondra pas [? ». Auwers (P. Donceel-Voûte, *cit.*, note 169) propose : *dalet-lamed-bet-dalet* plus loin *'alef-reš*-*'aîn*, plus loin *taw*. » Misgav, *cit.*, p. 431, lit : « w'dlbdl'r'kt ». Ces auteurs n'ont pas lu les lignes 1, ni 2'.

(16) Lemaire, *cit.*, p. 360–361, a lu : ?]YH/HDPŞ 'DSGDWT, et traduit : « ?].....prosternation? ». Auwers (P. Donceel-Voûte, *cit.*, note 169) propose : « *'alef-dalet-ṭet-reš-dalet-waw-he*, puis en bout de ligne *lamed* [mais ce dernier appartient à la ligne suivante]. Misgav, *cit.*, p. 431, lit : « whḥ?'dsgdwḥ ».

(17) Lemaire, *cit.*, p. 360–361, a lu : ?]BD-B DMW 'YYM/S 'MSTLM[?, et traduit : « ?]..... soyez tranquilles/silencieuses îles/côtes, je ferai fondre des collines[? ». Auwers (P. Donceel-Voûte, *cit.*, note 169) propose : « *bet-pe*-*'alef-nun* et vers la fin de la ligne *ṭet-taw*. » Misgav, *cit.*, p. 431, lit : « bdrbdm'n'dws'hstl ».

(18) Lemaire, *cit.*, p. 360–361, a lu : ?]QW W/Z/YR WRTṬW SSYMS/ML, et traduit : « ?].....secours et tremblez chevaux ? ... », comme si la ligne était incomplète. Auwers (P. Donceel-Voûte, *cit.*, note 169) propose : « un *'aîn* en début de ligne, vers les 2/3 *ṭet* puis *ḥet-lamed*. » Misgav, *cit.*, p. 431, lit : « sw'wdwrṭṭysrsl ».

(19) Lemaire, *cit.*, p. 360–361, a lu : ?]----B?RK----QDM[?, et traduit : « ?] d'autrefois[? ». Auwers (P. Donceel-Voûte, *cit.*, note 169) écrit : « rien n'a été identifiable ». Misgav, *cit.*, p. 431, lit : «yrld.....qws? ».

2.4. *Commentaire*

– Ligne 1 : La lecture *לְיָהֲשִׁתְמוּ עֵין כֹּל* n'est pas totalement assurée mais paraît vraisemblable, "...]ont bouché une source(?) tous[les ..., /...]ont bouché la source de tou(/te)s [les ...". Le mot *עֵין* peut être à l'absolu ou à l'état construit, et désigner un œil ou une source, voir 2 R 3,19.25 et 2 Ch 32,3–4, mais le verbe peut aussi signifier "ouvrir, déciller (un œil), creuser (un trou)", le contexte trop lacunaire ne permet pas d'être plus précis. Le verbe pourrait être un parfait avec l'interrogation "Jest-ce qu'ont bouché...".

– Ligne 2 : Les restes paraissent devoir être lus *lwmld*, verbe au *pi'el* (?), probablement à l'impératif. Cette lecture donne la fin d'une maxime de sagesse, de maîtrise de soi(?) ; le verbe *lmd* étant construit avec un double accusatif, la coordination de cette sentence suppose un autre verbe avec un objet ; à comprendre : "...] et enseigne à ton cœur : la mort n'est pas un mal !". Il s'agit peut-être d'entraîner et d'accoutumer son cœur(= soi-même) à sa fin. En conséquence, tu n'as pas à craindre la mort même si elle vient à l'improviste, car créature, tout homme est mortel. La mort peut parfois être considérée comme un bien, Tb 3,6, Jb 7,15 et Jr 8,3.

– Ligne 2' : Entre les lignes 2 et 3, on peut encore lire *תקבר כל מיתם טרם לילה כאשר צוה מ[יש]*, "...ensevelis chaque mort avant la nuit *comme a ordonné M[oïse]*". (20) On peut renvoyer à Dt 21,23 à propos du suspendu sur le bois qu'on doit ensevelir le jour-même de l'exécution, son cadavre ne peut pas être laissé la nuit sur le bois. Voir aussi Tb 2,4–7 où Tobit ensevelit les morts en cachette au coucher du soleil. Ici, la sentence vise les morts en général.

– Ligne 3 : Comme une lecture *yḥdw/yt/m* ne donne pas de sens, seules les lectures *whḵym* ou difficilement *yhbym* peuvent être retenues. Lire soit *lyhbym* au participe pluriel "jils donnent", ou mieux *lwhḵym* (de *ḥkh*) "jet ils attendent", mais le *hif'il* *lyḥkym* "il rendra sage" ne convient pas avec la suite, à moins d'une phrase en araméen *dly ḥkym* "d]u sage" mais 'k n'est pas connu en araméen. La séquence *lwhḵym 'k sgdw* peut donc se comprendre "...jet ils attendent uniquement des prosternations", visant les rois, les chefs, les sages, les scribes, les pharisiens, voir Mt 23,7, Mc 12,38, Lc 11,43 ; 20,46, etc. La séquence *lyhbym 'k sgdw* "jils donnent seulement des prosternations" paraît moins attendue, même en s'appliquant aux esclaves, aux pauvres, etc. En araméen, *sgdh* désigne "un lieu d'adoration/de proskynèse", ici au pluriel, alors que *sgdw* désigne la "proskynèse/adoration" elle même, ici au singulier ou au pluriel. On comprendrait plus difficilement des "idoles".

(20) Restes non lus dans l'édition, Lemaire, *cit.*, p. 360.

– Ligne 4 : La lecture [עובו עבִימ האך פס אם סתו למ] paraît tout à fait recevable. La séquence “[les nuages ont quitté/disparaissent]” est suivie d’une question, “est-ce assurément”. Puis la séquence *ps ’m stw* doit désigner “la disparition/cessation avec l’hiver” (sans doute la racine *pss* se construit avec *mn*, mais le *mem* final est rattaché à *’alef*), on peut donc difficilement lire le substantif *ps*’ avec *’alef* au lieu de *he*. On doit lire *ps ’m stw* avec le *mem* final, phonétiquement pour *’m* “avec”, (21) et à la cassure *lm*[devrait pouvoir être restauré *lm[tr*, “pour la p[luie”. Le mot *stw* est connu de Ct 2,11 dans une phrase tout à fait parallèle. En effet, la disparition des nuages est bien le signe de la cessation de la pluie à la fin de l’hiver. La phrase n’est pas d’une construction limpide mais elle est compréhensible. On peut comparer le dicton de *Cant. Rabba* à II 11 : *wl’ hw’ hgšm wl’ hw’ hstw* “la pluie et l’hiver ne sont-ils pas la même chose ?”

– Ligne 5 : La lecture paraît aussi assurée avec des traces à la cassure : [צ.מקו עור ובעטו סס לסל]. Précédant le *qof*, des traces conviennent au tracé de *mem*, ce qui recommande de restaurer *šmqw* “ils ont desséché/ridé” suivi du complément *’wr* “la peau”. Le sujet au pluriel peut renvoyer au soleil/à la chaleur et au vent chaud, voir le couple en Is 49,10 : *שרב ושמש*. Aussi comprendrait-on e.g. “le vent brûlant et le soleil ont [desséché/ridé la peau”, le verbe est bien connu en araméen. Ensuite on ne peut hésiter entre *wb’ṭ* et *wm’ṭ*, le tracé est celui de *bet*, *b’ṭ* “pousser, repousser” est lui aussi bien connu en araméen. La dernière séquence *ss lsl* ne fait pas de difficulté : *ss* (22) est généralement traduit par “teigne/mite” mais pourrait aussi désigner la vermine en général qui s’attaque “au panier” *sl* (et à son contenu ?). Ce n’est pas sans rappeler des maximes populaires des *Proverbes d’Aḥiqar* : 119 “Avec la fin de la nuit] la mite est arrêtée mais dès le soir[elle reprend son œuvre”, et 121 “Dans une maison de bronze la mite est arrêtée,[elle n’a pu percer un trou” (mes restaurations). La mite appartient aussi à la liste des bêtes qui dévorent dans les malédictions des traités araméens de Sfiré (KAI 222 A 31). Les parfaits sont des parfaits proverbiaux ou gnomiques.

– Ligne 6 : La cassure de la pierre nous prive d’une partie de la sentence. Cette partie de maxime devrait pouvoir se comprendre ainsi : [כ.לב שען]ר טרד וי>תוע קדם צאן, “Un ch]ien a chassé le bouc

(21) Pour l’emploi du *’alef* pour le *’ain* à Qumrân, y compris de la préposition *’m* pour *’m*, voir IQS VIII 2 : *’yš ’m r’hw* “chacun avec son prochain”, tout comme il en va de *’l* pour *’l*, voir IQS VII 2, X 1, 4Q175 7, etc. Sur ces interchanges de consonnes gutturales, voir E. Qimron, *A Grammar of the Hebrew of the Dead Sea Scrolls, Between Bible and Mishnah* (Jerusalem: Yad Yizhaq Ben-Zvi, 2018), p. 106.

(22) Lemaire, *cit.*, p. 361, comprend *ssym* “chevaux”, mais cela est exclu, d’autant que à cette époque le mot serait orthographié *swsym*.

et il a erré devant le t[r]oupeau (de chèvres)”. (23) Prophétisant le retour des exilés, Jr 50,8 use d’une image comparable : “Sortez, soyez comme des boucs en tête d’un troupeau”.

Ces quelques lignes parfois assez maladroitement écrites sont celles d’un apprenti scribe réutilisant la pierre qui porte encore des traces de lettres mal effacées avec de l’eau, puisque entre les lignes 2 et 3, on peut encore lire une sentence assez lisible (ligne 2’). Mais il y a des restes d’écriture un peu partout, entre les lignes 3 et 4, 4 et 5, 5 et 6 et au-dessous de 6. De la pierre n’est conservée en grande partie que la partie gauche, puisque la fin des maximes des lignes 2, 3, (4) et 5 sont assurées, et la ligne 6 se comprend aussi de la fin de la phrase. Mais font défaut les débuts des lignes, même si des restaurations peuvent permettre de comprendre les phrases ou des parties de phrases. Le scribe paraît avoir copié une sentence par ligne, ce qui souligne un certain degré d’application dans son exercice d’apprenti. Quoi qu’il en soit, la lecture des lettres d’un apprenti est moins désespérée que ce qu’en ont dit les auteurs. Et les phrases n’ont rien de connotation magique. (24) On peut comparer cet apprentissage à des exemples araméens sur des bols retrouvés à Marésha. (25)

Une datation paléographique reste aléatoire concernant un exercice de scribe en écriture semi-cursive, qui plus est, sur une surface parsemée d’aspérités rendant les tracés irréguliers, le calame accroche et peut laisser comme des tracés discontinus (voir le *mem* final, ligne 4). La main du scribe est moins sûre et régulière que d’aucun l’a prétendu, il s’agit bien d’un apprentissage d’écriture à l’aide de phrases ou sentences variées et non d’une séquence des lettres de l’alphabet, (voir figure 3 pour la comparaison des deux types d’écriture). Toutefois la découverte de la pierre en surface au *locus* 129 pourrait suggérer plutôt une attribution dans la première moitié du premier siècle de notre ère, lors des dernières générations esséniennes, soit au plus tard vers le milieu du premier siècle, la graphie est d’époque hérodienne. (26) Il faut encore rappeler que dans le *locus* 129 a aussi été découvert un encrier *KhQ* 4338 trouvé le 27/3/55, mais attribué dernièrement au

(23) Philon d’Alexandrie nous renseigne quelque peu sur les Esséniens, *Apologie des Juifs* § 8 : « Parmi eux, il y a des agriculteurs versés dans l’art d’ensemencer et de travailler la terre, des pasteurs qui conduisent toute sorte de troupeaux ; quelques uns s’occupent d’apiculture ». Cela ne fait pas pour autant du *locus* 129 une bergerie, mais ont été retrouvés sur le site des instruments agricoles, socs de charrue, serpes, houes, etc.

(24) Voir Misgav, *cit.* p. 461.

(25) Voir E. Eshel, E. Puech and A. Kloner, « Aramaic Scribal Exercises of the Hellenistic Period from Maresha: Bowls A and B », *BASOR*, 345 (2007) 39–62.

(26) Lemaire, *cit.* p. 361, la date approximativement “vers le tournant de notre ère”.

Conclusion

Ces témoignages d'apprentissage d'écriture en langue hébraïque, un élémentaire sur un ostracon qui n'est pas un abécédaire (28) et des copies de sentences sur une pierre de calcaire, retrouvés dans les ruines du site sont à mettre en rapport avec des témoignages d'apprentissages tout à fait comparables retrouvés dans les grottes aux alentours de la *khirbeh*. (29) Avec les nombreux fragments de manuscrits ces derniers prouvent une intense activité scribale à Sokoka-Qumrân. La présence de plusieurs encriers sur le site, de l'ostracon de vente d'une propriété à Jéricho par un nouvel entrant dans la Communauté (30) ainsi que d'autres documents écrits retrouvés dans la ruine corroborent la relation étroite des occupants du site avec le contenu des grottes qui ne sont pas des *genizot*. (31) Ils prouvent la présence d'une communauté essénienne dès la réoccupation du site au milieu du deuxième siècle av. J.-C. En effet, l'élémentaire tout comme la pierre ne sont pas des objets importés, mais des témoins d'apprentis scribes sur place. (32) L'élémentaire datant de la fin de l'époque hasmonéenne entre parfaitement dans l'interprétation de l'occupation de R. de Vaux à la période 1B, mais s'il se comprendrait très difficilement d'un membre d'un camp de réfugiés, il ne peut pas s'expliquer dans l'hypothèse d'un centre de pèlerinage à Sokoka-Qumrân à cette époque-là. (33)

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Post-scriptum :

Ligne 3 : Au lieu de lire והכות, lire והכות, au féminin pluriel jambage du *taw* bouclé et tête de *waw* qui recouvre une partie de l'épaule du *taw*.

(28) Lemaire, *cit.*, p. 341–342.

(29) Voir White Crawford, *cit.*, p. 216–218, pour la liste ; des alphabets sont suivis d'anthroponymes commençant par des lettres selon l'ordre alphabétique, etc.

(30) Voir E. Puech, « L'ostracon de *Khirbet Qumrân* (KhQ 1996/1) et une vente de terrain à Jéricho, témoin de l'occupation essénienne à Qumrân », in *Flores Florentino. Dead Sea Scrolls and Other Early Jewish Studies in Honour of Florentino García Martínez*, ed. by A. Hilhorst, E. Puech and E. Tigchelaar, JSJSup. 122 (Leiden/Boston: Brill, 2007), 1–29.

(31) L'étude d'autres documents écrits retrouvés sur le site fera l'objet d'une note complémentaire.

(32) Ces deux témoins de l'apprentissage d'écriture en hébreu s'opposent à la conclusion de Misgav, *cit.*, p. 435–36, en faveur de la présence d'une population locale de langue araméenne occupée à des travaux agricoles et à la vente de leurs produits. En revanche, ils témoignent d'autres préoccupations en relation directe avec les manuscrits retrouvés dans les grottes.

(33) Voir J.-B. Humbert, « Reconsidération of the Archaeological Interpretation », in *Khirbet Qumrân et 'Ain Feshkha. II, op. cit.*, p. 419–444.

THE POST-2002 FRAGMENTS’ DEPENDENCY ON MODERN EDITIONS OF THE HEBREW BIBLE (1)

Abstract

This article presents the results of a systematic analysis of 27 unprovenanced post-2002 “Dead Sea Scrolls” fragments, the goal of which has been to test a hypothesis of textual correspondence between fragments and modern editions of the Hebrew Bible. The hypothesis is twofold: (1) There is a line-to-line layout correspondence between some fragments and modern editions of the Hebrew Bible; (2) readings suggested in the critical apparatus by the editors of the modern editions often appear to have been imported onto the fragments. The analysis confirms that six of ten fragments which were known to be modern forgeries at the time this analysis was conducted, as well as five of the remaining seventeen fragments, exhibit this feature. The article therefore illustrates that textual correspondence is in some cases a characteristic of modern forgery, and that some forgeries attest to a banal use of modern editions of the Hebrew Bible in the forgers’ fragment production.

INTRODUCTION

OVER the past several years, the authenticity of several unprovenanced fragments purported to be both ancient and from the area around the Dead Sea has been called into question. (2)

(1) This article is based on research completed as part of a MA thesis at MF Norwegian School of Theology, Religion and Society, Oslo. Thanks are due to Matthew Monger, Årstein Justnes and Torleif Elgvin for their guidance and feedback. I am also grateful for the feedback and comments from the three anonymous reviewers.

(2) Kipp Davis, Ira Rabin, Ines Feldman, Myriam Krutzsch, Hasia Rimon, Årstein Justnes, Torleif Elgvin, and Michael Langlois, “Nine Dubious ‘Dead Sea Scrolls’ Fragments from the Twenty-First Century,” *Dead Sea Discoveries* 24 (2017): 189–228, doi:10.1163/15685179-12341428; “Museum of the Bible Releases Research Findings on Fragments in Its Dead Sea Scrolls Collection,” *Museumofthebible.org*, 22 October

It is important to mention that when dealing with unprovenanced material, one should first and foremost address provenance issues. (3) Dennis Mizzi and Jodi Magness rightly argue that scholars should not publish unprovenanced material, and further specify that even after primary publication, one should only research unprovenanced fragments if the goal is “to highlight the fact that an already published fragment is a fake or else lacks the necessary information to prove whether or not it is licit.” (4) This article aims to illustrate the post-2002 fragments’ dependency on modern editions of the Hebrew Bible, and thus aligns with the aforementioned goal.

Physical and paleographic features of some of the post-2002 “Dead Sea Scrolls” (“DSS”) fragments have been evaluated in order to ascertain their authenticity. (5) Another feature which is allegedly found in post-2002 fragments concerns the text and layout of the fragments. (6) In this article, I will present and evaluate the hypothesis that some post-2002 “DSS” fragments show significant textual correspondence to modern editions of the Hebrew Bible and discuss what this could entail. This builds on Christian Askeland’s work on “the Gospel of Jesus’ Wife” (7) as well as studies by Kipp Davis, Eibert Tigchelaar, Årstein Justnes, and Torleif Elgvin. (8) It is my hope that this article can contribute with insights into the forgery production of DSS.

2018, https://www.museumofthebible.org/press/press-releases/museum-of-the-bible-releases-research-findings-on-fragments-in-its-dead-sea-scrolls-collection?fbclid=IwAR0abWh-Q125hQ-yRPM1fTit3sLMmET80Ku338_xXJYpVxGcLfa7N5qdT0k0.

(3) For a succinct and well-presented argument of this, see Dennis Mizzi and Jodi Magness, “Provenance vs. Authenticity: An Archeological Perspective on the Post-2002 ‘Dead Sea Scrolls-like’ fragments,” *Dead Sea Discoveries* 26 (2019): 135–169, doi:10.1163/15685179–12341503.

(4) Mizzi and Magness, “Provenance vs. Authenticity,” 159.

(5) Davis et al., “Nine Dubious”; Torleif Elgvin and Michael Langlois, “Looking back,” *Revue de Qumran* 31 (2019): 111–133, doi: 10.2143/RQ.31.1.3286506; Eibert J. C. Tigchelaar, “Gleanings from the Caves? Really? On the Likelihood of Dead Sea Scrolls Forgeries in the Schøyen Collection,” *Revue de Qumran* 29 (2017) 314–322.

(6) See for example Justnes and Elgvin’s useful insights regarding textual correspondence between fragments and modern text editions: Årstein Justnes and Torleif Elgvin, “A Private Part of Enoch: A Forged Fragment of 1 Enoch 8:4–9:3,” in *Wisdom Poured Out Like Water: Studies on Jewish and Christian Antiquity in Honor of Gabriele Boccacini*, edited by J. Harold Eddens et al. (Berlin: de Gruyter, 2018), 195–203. See especially pp. 200–203.

(7) Christian Askeland, “A Fake Coptic John and its implications for the ‘Gospel of Jesus’s Wife’,” *Tyndale Bulletin* 65 no. 1 (2014): 1–10.

(8) See for example Davis et al., “Nine Dubious”; Kipp Davis, “Caves of Dispute: Patterns of Correspondence and Suspicion in the Post-2002 ‘Dead Sea Scrolls’ Fragments,” *Dead Sea Discoveries* 24 (2017): 229–270, doi:10.1163/15685179–12341441; Eibert J. C. Tigchelaar, “A Provisional List of Unprovenanced, Twenty-First Century,

The hypothesis of textual correspondence is twofold: 1) It argues that there is a correspondence in line to line layout between certain fragments and modern editions of the Hebrew Bible; (9) and 2) that there is a tendency to import readings suggested in the critical apparatus by the editors of the modern editions. (10) The hypothesis is thus that potential forgers were in some cases very reliant on the texts of modern editions of the Hebrew Bible for the production of forgeries. For the layout of the fragments this seems to be a practical question, but when it comes to the textual variants, the motivation for, and importance of, the use of the text-critical apparatus of a modern edition is more complicated. The presence of variants which are not known from Hebrew manuscripts, but are found in other ancient translations, or even only as suggestions by the editors of a modern edition, may have contributed to scholars' fascination with these fragments.

SELECTED FRAGMENTS

In the following, I will present the results of a systematic analysis of twenty-seven fragments from The Schøyen Collection and the Museum of the Bible Collection (MOTB). (11) Ten of these fragments were already known to be modern forgeries at the time this analysis was conducted (12). As was the case for "the Gospel of Jesus' Wife," (13) six of these ten fragments confirm the hypothesis of textual correspondence.

Dead Sea Scrolls-like Fragments," *Dead Sea Discoveries* 24 (2017): 173–188; Justnes and Elgvin, "A Private Part of Enoch."

(9) Torleif Elgvin, "Texts and Artefacts from the Judaean Desert in The Schøyen Collection: An Overview," in *Gleanings from the Caves: Dead Sea Scrolls and Artefacts from The Schøyen Collection*, ed. Torleif Elgvin, Kipp Davis, and Michael Langlois, LSTS 71 (London: Bloomsbury T&T Clark, 2016), 53. See also Askeland, "A Fake Coptic John," where Askeland argues convincingly for the inauthenticity of a (fake) Coptic John fragment, and by extension The Gospel of Jesus' Wife, based on its "textual affinity with the 1924 edition of the 'Qau codex'."

(10) Elgvin, "Texts and Artefacts," 53; Davis et al., "Nine Dubious," 201.

(11) The selection of the material here is influenced by access to research on the fragments in The Schøyen Collection and the MOTB.

(12) Davis et al., "Nine Dubious"; "Museum of the Bible Releases Research Findings." Elgvin and Langlois recently published an article ("Looking back") presenting their current position on the authenticity of the fragments and artefacts in the 2016 publication of The Schøyen Collection, deeming many of them modern forgeries. Note that "Looking back" was published after this article was written. It was therefore not possible to incorporate their designations in this article.

(13) Askeland, "A Fake Coptic John."

To avoid confusion of the different numbering systems in the two collections, Tigchelaar's numeric system will be used. (14) However, because the fragments' status as DSS fragments is so dubious, the initial "DSS" in Tigchelaar's system will be excluded. Furthermore, only fragments which contain biblical texts which are a part of the traditional Hebrew canon have been included in the analysis. (15) This means that books such as Tobit and 1 Enoch are left out of this particular analysis – as are fragments in other collections. The only exception from the selection above is F.193 which, due to its lack of concrete identification, was too difficult to analyze in the scope of this article. (16) For the sake of testing the assumption of this article empirically, an equal number of "pre-2002" fragments have also been analyzed using the same criteria, and the results have been compared. (17) These results will not be presented in detail, but will be mentioned briefly, so as to verify the criteria used in this analysis.

SELECTED EDITIONS

In this analysis, Kittel's second edition of *Biblia Hebraica* (18) (BHK) and *Biblia Hebraica Stuttgartensia* (19) (BHS) will be consulted. This selection has been made because Torleif Elgvin and Kipp Davis have named these editions as possible bases for some of the fragments in question. (20) Furthermore, BHK and BHS are the most common tools in the field, (21) and cover the two main textual bases for modern

(14) Tigchelaar, "A Provisional List," 178–184.

(15) A complete list of all the post-2002 fragments analyzed for this article is found in the appendix.

(16) *Ibid.*, 182.

(17) The fragments are of roughly the same size as the post-2002 fragments in question and have been chosen to cover most of the books from the Hebrew Bible (as the post-2002 fragments do), but otherwise at random. The following "pre-2002" fragments were analyzed: 1QGen frg 1, 1QGen frg 2, 1QExod frg 1, 1QDeut frg 9, 4QLev^c frg 1, 4QLev^c frg 3, 4QDeut^c frg 1, 4QDeut^c frg 5, 4QJosh^a frg 17–18, 4QJudg^b frg 1, 4QKgs frg 1, 4QKgs frg 3–4, 4QIsa^a frg 6, 4QIsa^a frg 9, 4QJer^a col 6 part 1, 4QJer^a col 9 part 2, 4QJer^b, 4QEzek^a frg 3 col 2, 4QPs^a frg 2, 4QPs^b frg 5 col 2, 4QJob^b frg 1, 4QJob^b frg 4, 4QProv^b frg 1, 4QRuth^b frg 4, 4QLam col 1 frg 1, 4QDan^a frg 7, and 4QEzra frg 2.

(18) Rudolf Kittel, ed., *Biblia Hebraica*, 2nd edition, volume I-II (Stuttgart: Privileg. Württ. Bibelanstalt, 1925).

(19) K. Elliger and W. Rudolph, ed., *Biblia Hebraica Stuttgartensia* (Stuttgart: Deutsche Bibelgesellschaft, 1997).

(20) Elgvin, "Texts and Artefacts," 53; Davis, "Caves of Dispute," 259.

(21) The Hebrew University Bible (HUB) is too late for the fragments in this analysis, but *Biblia Hebraica Quinta* (BHQ) should be consulted in future analyses.

editions of the Hebrew Bible, namely the second Rabbinic Bible (RB2) and the Leningrad Codex (L). (22) In short, BHK and BHS allow for a thorough analysis covering the two likely groups of editions which potential forgers may have turned to. For a later analysis, DJD should also be consulted. (23)

STRUCTURE OF ANALYSIS

In this article, the fragments have been divided into three categories: First, fragments which were known to be modern forgeries at the time this article was written (see below) will be addressed. Secondly, fragments about which concerns were raised in the official publications of The Schøyen Collection (*Gleanings from the Caves: Dead Sea Scrolls and Artefacts from The Schøyen Collection [Gleanings]*) (24) and the MOTB Collection (*Dead Sea Scrolls Fragments in the Museum Collection [DSSFMC]*) (25), will be analyzed. Lastly, the remaining fragments in The Schøyen Collection and the MOTB Collection will be analyzed. (26) The two first categories are explained more thoroughly below. Only for fragments exhibiting clear signs of textual correspondence will the individual analyses be presented in this article.

Fragments known to be modern forgeries

The article “Nine Dubious “Dead Sea Scrolls” Fragments from the 21st Century” (*Nine Dubious*) provides “evidence that nine of these Dead Sea Scrolls-like fragments are modern forgeries.” (27) It contains an analysis of the following fragments: F.103 (Exod), F.104 (Exod), F.105 (Exod), F.112 (1 Sam), and F.122 (Neh) in addition to three Enoch fragments and one Tobit fragment. These were, due to their dubious nature, “withheld from *Gleanings from the Caves*, the official

(22) Emanuel Tov, *Textual Criticism of the Hebrew Bible*, 3rd edition (Minneapolis: Fortress Press, 2012), 70–73.

(23) Justnes and Elgvin, “A Private Part of Enoch,” 201–202; Davis et al., “Nine Dubious,” 203.

(24) Elgvin, Davis, and Langlois, ed., *Gleanings*.

(25) Emanuel Tov, Kipp Davis, and Robert Duke, ed., *DSSFMC*, Publications of Museum of the Bible 1 (Leiden: Brill, 2016).

(26) Photographs of all MOTB fragments in question can be found in *DSSFMC*. Photographs of all Schøyen fragments in question, except those that were withheld from the official publication, can be found in *Gleanings*. Photographs of the five fragments which were withheld from *Gleanings* can be found in Davis et al., “Nine Dubious.”

(27) Davis et al., “Nine Dubious,” 190.

publication of scrolls and artifacts in The Schøyen Collection.” (28) The article argues strongly that these fragments are modern forgeries.

On October 22nd 2018, MOTB published a press release announcing that five of its fragments are modern forgeries: “Utilizing leading-edge technology, the German-based Bundesanstalt für Materialforschung und -prüfung (BAM) has performed a battery of tests and concluded that the five fragments show characteristics inconsistent with ancient origin and therefore will no longer be displayed at the museum.” (29) The press release did not mention the problematic aspects of unprovenanced material in and of itself, but focused on the question of authenticity. Although official reports on the fragments have not been published, David Bradnick named the five fragments which the press release concerns in a response to a Twitter post by *Biblical Archeology Review* (BAR). They are F.191 (Gen), F.194 (Num), F.197 (Jon), F.201 (Neh), and F.203 (Lev). (30)

It is clear that the fragments mentioned above are modern forgeries. The ten fragments will therefore be addressed as *Fragments known to be modern forgeries*. Elgvin and Langlois have in their recent assessment categorized all Schøyen fragments in this analysis, except F.114 (2 Sam), as fake. (31) However, as “Looking back” was published after this article was written, only the fragments that were withheld from publication, or were later announced by their current collector to be fake, will be included under *Fragments known to be modern forgeries*.

Fragments about which concerns were raised in The Schøyen Collection and the Museum of the Bible Collection

In addition to fragments that were withheld from the publication of *Gleanings*, and the five fragments figuring in MOTB’s press release, there are several fragments about which concerns were raised in the official publications of the two collections. What this entails is that the editors of the official publications have voiced concerns regarding the fragments’ authenticity but decided to publish the fragments in question. (32)

(28) Ibid.

(29) “Museum of the Bible Releases Research Findings.”

(30) David Bradnick, “Five Museum of the Bible Dead Sea Scrolls Are Fake,” Twitter, 28 October 2018, https://twitter.com/d_bradnick/status/1056767332268367872.

(31) Elgvin and Langlois, “Looking back,” pp. 130–132.

(32) Note, however, that for the following fourteen fragments, Michael Langlois has described the hand as hesitant, inconsistent or otherwise irregular: F.101 (Gen), F.103 (Exod), F.104 (Exod), F.105 (Exod), F.107 (Num) F.108 (Deut), F.112 (1 Sam),

Criteria for Analysis

Because this type of systematic analysis on “DSS” fragments has never been published before, no formal criteria determining textual correspondence have been established. For the sake of consistency in the analysis, these needed to be formed. When determining whether a fragment’s layout corresponds to modern editions, four categories have been utilized. These four categories are:

- Close match: This term is used when there is a striking resemblance between the layout of a fragment and the layout in one or more of the modern editions. This entails that words and letters are in approximately the same relative position to each other on the fragment as they are in the modern editions.
- Consistent shift: This term is used when each line begins one line below the point at which the previous line ended.
- Some correspondence: This term is used when there is a less obvious, but still visible, correspondence between the layout of a fragment and the layout in one or more of the modern editions.
- No correspondence: This term is used when there is no visible correspondence between the layout of a fragment and the layout in any of the modern editions.

As for variants, the goal of mapping their presence or absence in the critical apparatus is to test the hypothesis of imported readings. There is no infallible way of determining whether readings are suspicious or not, but the arguments presented in “Caves of Dispute” (33) and “Nine Dubious” (34) are very convincing. In light of this, the following categories will be used when analyzing variants:

- Notable: This term is used when hitherto unwitnessed readings which have been suggested in the critical apparatus are found on the fragment.
- Potentially notable: This term is used when I felt that the evidence was ambiguous. This may for example relate to a short text which includes a great number and variety of variants.

F.113 (1 Sam), F.115 (1 Kgs), F.116 (Jer), F.118 (Ps), F.119 (Prov), F.120 (Ruth), and F.122 (Neh). Certain fragments, e.g. F.101 (Gen), also seem to contain text which mixes older and younger palaeographical features. See Langlois, “Paleographical Analysis of the Dead Sea Scrolls in The Schøyen Collection,” pages 79–128 in *Gleanings*. And Davis et al., “Nine Dubious.”

(33) Davis, “Caves of Dispute.”

(34) Davis et al., “Nine Dubious.”

- Not considered notable: This term is used when there is nothing which indicates imported readings. The term will also be used when the results are inconclusive, e.g. when the text is too fragmentary to determine variants.

The results of the analysis of “pre-2002” fragments indicate that *some layout correspondence* and the presence of *potentially notable variants* can be coincidental, as these elements are also present in several pre-2002 fragments. (35) Therefore, only fragments exhibiting a close match, a consistent shift and/or notable variants will be referred to as exhibiting textual correspondence to modern editions, and thus be presented in detail below.

It is my opinion that when a fragment exhibits a close match in layout or a notable variant, this could indicate that the forger who produced the fragments simply copied the text from modern editions, sometimes importing readings from the critical apparatus, without editing the layout of the text, thus not differentiating it from the modern edition in question. (36) One can speculate that more sophisticated forgers may have made changes to the texts’ readings and layout prior to inscribing the fragment, in order not to raise suspicions about the fragments’ authenticity.

ANALYSIS OF FRAGMENTS KNOWN TO BE MODERN FORGERIES

Ten fragments in this analysis are known to be modern forgeries. Five of these belong to the MOTB Collection (F.191 [Gen], F.194 [Num], F.197 [Jon], F.201 [Neh], and F.203 [Lev]), and five are part

(35) Of the 27 pre-2002 fragments which were analyzed (see footnote 17 for list of pre-2002 fragments), three exhibit a close match (4QDeut^c frg. 5, 4QJer^a col. 6 part 1, and 4QProv^b frg. 1), five exhibit some correspondence (1QExod frg. 1, 4QLev^c frg. 3, 4QJudg^b frg. 1, 4QPs^b frg. 5 col. 2, and 4QJob^b frg. 1), none exhibit a consistent shift, and in 19 there is no layout correspondence. This means that where 66,6 % of the post-2002 fragments known to be modern forgeries exhibit a close match or a consistent shift in layout, and 33,3 of all the 27 post-2002 fragments analyzed do the same, this is only found in 11,1 % of the 27 pre-2002 fragments. In other words, the occurrence of a consistent shift or a close match is much more frequent in the post-2002 than the pre-2002 fragments. None of the pre-2002 fragments contained notable variants, two were hesitantly categorized as potentially notable (4QPs^b frg. 5 col. 2 and 4QEzra frg. 2), and 25 contained no notable variants. The presence of notable variants is in other words of interest, as it is only found in the post-2002 fragments in this analysis.

(36) This, in turn, obviously entails that the text on said fragments must have been inscribed after the publication of these modern editions, and not in the times they are purported to have originated.

of The Schøyen Collection (F.103 [Exod], F.104 [Exod], F.105 [Exod], F.112 [1 Sam], and F.122 [Neh]). (37)

F.103 (Schøyen, MS4612/2a, Exod3, Exod 3:13/14–15) (38)

Layout correspondence: close match with 4QExod^b

Transcription (39)

BHK, Exodus 3:13–15 (40)

א[1	וַיֹּאמֶר אֱלֹהִים: 14 וַיֹּאמֶר אֱלֹהִים אֶל
ר[לבני ישראל] 2	וַיֹּאמֶר כֹּה תֹאמַר לְבְנֵי יִשְׂרָאֵל אֲהִיָּהּ
ר[עוד אלהים א[3	וַיֹּאמֶר עוֹד אֱלֹהִים אֶל־מֹשֶׁה כֹּה תֹאמַר אֶל
הם ואלהי י[4	אֱלֹהֵי אֲבֹתֵיכֶם אֱלֹהֵי אַבְרָהָם אֱלֹהֵי יִצְחָק
	וְאֱלֹהֵי יַעֲקֹב וְהָיָה זִכְרִי לְדֹר דֹּר: 16 לָךְ וְ

The text on the fragment almost exclusively contains words, or parts of words, which are important in the Hebrew Bible. לבני ישראל (to Israel's sons), אלהים (God), אברהם (Abraham's), and אלהי יצחק (and Isaac's God) can most certainly be characterized as such. It may at first glance seem as though the forgers have chosen the most important terms in the middle of the page in BHK. However, I was made aware by Davis et al. that the layout of the fragment closely matches that of 4QExod^b. (41) It therefore seems that the forger has copied the text from a fragment published in DJD rather than from BHK or BHS. The fragment is categorized as a close match although it matches a DJD publication rather than a modern edition. (42)

(37) For an overview of the acquisition history, see Årstein Justnes and Ludvik A. Kjeldsberg, "The Post-2002 Dead Sea Scrolls Fragments: A Tentative Timeline of Acquisitions," *The Lying Pen of Scribes: Manuscript Forgeries and Counterfeiting Scripture in the Twenty-First Century*, 24 November 2018, <https://lyingpen.com/>.

(38) A photo of the fragment can be found in Davis et al., "Nine Dubious," 203.

(39) Thanks are due to Torleif Elgvin and Årstein Justnes for giving me access to their transcription from May 2013 (unpublished).

(40) Kittel ed., *Biblia Hebraica*, Exodus 3:13–15.

(41) Davis et al., "Nine Dubious," 203; Justnes and Elgvin, "A Private Part of Enoch," 201–202. For comparison, see Eugene Ulrich, Frank Moore Cross, James R. Davila, Nathan Jastram, Judith E. Sanderson, Emanuel Tov, and John Strugnell, *Qumran Cave 4 VII, Discoveries in the Judean Desert 12* (Oxford: Clarendon Press, 1994), 90.

(42) This correspondence to 4QExod^b should urge scholars to conduct an analysis of textual correspondence between all post-2002 fragments and DJD publications.

F.112 (Schøyen, MS4612/10, Sam1, 1 Sam 2:11–14) (43)*Layout correspondence: consistent shift*

Transcription (44)

BHK, 1 Samuel 2:11–14 (45)

1	וְאֶלְמָנָה הָרִמְתָּה עַל־בֵּיתוֹ וְהִנֵּער הָיָה מִשְׁרַת אֶת־הָהָה	1000]
2	יְיָ עַל־הַכֹּהֵן: 12 וְיִבְנֶי עָלַי בְּנֵי בְלִיעֵל לֹא יִדְעוּ אֶת־הָהָה:	[לי בני בליעל לא]
3	פֶּטַח הַכֹּהֲנִים אֶת־הָעַם כָּל־אִישׁ זָבַח וְזָבַח וְזָבַח נֶעַר הַכֹּהֵן	1000] או נער הכהן]
4	הַבָּשָׂר יִהְיוּ לֶגְשָׁלָם שְׁלֹשׁ הַשָּׁנִים בְּיָדוֹ: 14 וְהָלָה בְּכִיזֹר אוֹ בְּיָדֹד	[או בקלחת או בפרור או]
	לְחַת אוֹ בְּפִרְזֹר כָּל אֲשֶׁר־יַעֲלֶה הַפֶּטַח יִקַּח הַכֹּהֵן בְּיָדוֹ בָּקָה	
	לְכָל־יִשְׂרָאֵל הַבָּאִים שָׁם בְּשָׁלָה: 15 וְגַם בְּטָרֶם יִקְטְרוּ אֶת־	

F.122 (Schøyen, MS5426, Neh1, Neh 3:14–15) (46)*Layout correspondence: close match*

Transcription (47)

BHK, Nehemiah 3:14–15 (48)

] Top margin

וְהָיָה כִּי יִבְנֶה בְּנֵי יִשְׂרָאֵל וְהָיָה כִּי יִבְנֶה בְּנֵי יִשְׂרָאֵל	
הוּא יִבְנֶנּוּ וַיַּעֲמִיד דְּלַתְתָּיו מִנְעָלָיו וְכָרַ	1 ובנו ועמיד דלתתיו ב]
הַחֲזִיק שַׁלְוִי בֶן־כָּל־חֹזֶה שָׂר פֶּלֶךְ הַמַּצָּו	2 שלום בן כל חזה שר חצ]
וַיַּעֲמִידוּ דְּלַתְתָּיו מִנְעָלָיו וּבְרִיחָיו וְאֵת ז	3 דלתתיו מנעולו •]
הַמֶּלֶךְ וְעַד־הַמַּעֲלֹת הַיְּהוּדוֹת מֵעִיר יְהוּדָה:	4 המלך] •[
בֶּן־עֲזַבּוּק שָׂר חָצִי פֶלֶךְ בֵּית־צֹר עַד־נִגְדֹ	

(43) A photo of the fragment can be found in Davis et al., “Nine Dubious,” 214.

(44) Thanks are due to Torleif Elgvin for giving me access to his transcription from January 2015 (unpublished).

(45) The first line is a tentative guess as only traces of three indiscernible letters are visible. See Kittel ed., *Biblia Hebraica*, 1 Samuel 2:11–14.

(46) A photo of the fragment can be found in Davis et al., “Nine Dubious,” 222.

(47) Thanks are due to Torleif Elgvin for giving me access to his transcription from April 2014 (unpublished).

(48) Kittel, ed., *Biblia Hebraica*, Nehemiah 3:14–15.

Each line on the fragment starts by the right margin, whereas the lines shift ever so slightly towards the left in BHK due to the presence of one word (two on the last line) before the starting word on each line. One cannot deny, however, that the layout correspondence is a close match.

Notable variants

יבננו (*and his sons*) differs from מ which reads יבנו (*he builds it*), but corresponds to ט which reads και ος ιοις αυτου. If the transcription is correct, the correction of the supralinear *nun* results in יבננו (*and our sons*). This does not correspond with any reported textual witnesses. Visually, it admittedly brings the text closer to מ. It is also conceivable that an initial *waw* could later have been confused with a *yod*. In this respect, the text on the fragment can be understood as a textual link between ט and מ. This may be the theory behind Charlesworth's suggestion that the supralinear *nun* has been inserted to make the text "more in line with the so-called MT". (49) Such corrections are, after all, a well-known practice. However, as this fragment is a modern forgery, it must not be mistaken as a genuine stage in the history of the text. On the contrary, it prompts an important question: *Are theories created based on observations of the fragments, or are the fragments created based on theories about the origin of variant readings and development of texts?* (50)

The reading of the name שלום (*Shallum*), matches a few ט manuscripts, but differs from מ. The most interesting variant is the likely presence of חצי (*half*) between שר (*leader/commander*) and פלך (*district*), which corresponds to a suggestion made in the critical apparatus of BHS, but which is not found in any known textual witnesses. Lastly, the fragment contains the hitherto unwitnessed singular form מנעול (*his lock*), as opposed to the plural form in מנעול.

(49) Charlesworth, "Unpublished Dead Sea Scrolls Nehemiah."

(50) According to Elgvin (personal communication), F.122 was probably written to appear as if it belonged to 4QEzra. However, the fragment differed enough from 4QEzra that Elgvin determined it could not have belonged to the same scroll. Furthermore, F.122 is a more independent text than the relatively מ-like 4QEzra. See Eugene Ulrich, Frank Moore Cross, Joseph A. Fitzmyer, Peter W. Flint, Sarianna Metso, Catherine M. Murphy, Curt Niccum, Patrick W. Skehan, Emanuel Tov, and Julio Trebolle Barrera, *Qumran Cave 4 XI: Psalms to Chronicles*, Discoveries in the Judean Desert 16 (Oxford: Clarendon Press, 2000), 291–293.

Layout correspondence: close match

Transcription (52)	BHS, Numbers 8:3-5 (53)
[וְשָׂר צוֹה]	הַמִּזְבֵּחַ: 3 וַיַּעַשׂ כֵּן אֹהֲלֵי אֵל מִלִּפְנֵי הַמִּזְבֵּחַ הַשֵּׁלִישִׁי נִתְּנָהּ כַּאֲשֶׁר
יְרִיכָה וְעַד פְּרָחִי]	צִוָּה: יְהוָה אֶת מֹשֶׁה: 4 וְיָזָה מִעֲשֵׂה הַמִּזְבֵּחַ מִקִּשְׁתֹּךְ וְהָב עֲדֵי־יְרִיכָהּ
מֹשֶׁה כֵּן עָשָׂה אֶת הַמִּזְבֵּחַ]	עֲדֵי־פְרָחִי: הָיָא כַּמְרָאָה אֲשֶׁר הָרָאָה יְהוָה אֶת־מֹשֶׁה בֶּן
וַיְדַבֵּר יְהוָה אֶל־מֹשֶׁה לֵאמֹר:	עָשָׂה אֶת־הַמִּזְבֵּחַ: 5
לֵאמֹר: 6 כָּחֵף אֶת־הַלֵּלִים מִחוּדֹךְ בְּנֵי־	יִשְׂרָאֵל וְשִׁמְרֵם אֹתָם: 7 וְלֵבָב מִעֲשֵׂה לֶחֶם לְשֹׁמְרִים הָיָה עֲלֵיהֶם כִּי

F.197 (MOTB, SCR.003171, Jon1, Jon 4:2-5) (54)

Layout correspondence: close match

Transcription (55)	BHS, Jonah 4:2–5 (56)
<p>על הרעה ועלה]</p> <p>[ותי מחיי]</p> <p>[כה יוצא יונה מן העיר ויוש]</p> <p>[ושב] כה בצל עד אשר</p>	<p>אֲפִלִּים וְרִבְחֵסֶד וְנִחָם עַל־הָרָעָה: ³ וַתֵּקֶה יוֹנָה קִדְחָא אֶת־נַפְשָׁא</p> <p>מִמֶּיּוּ כִּי מָלֵב מוֹתִי מִמֶּיּוּ: ⁴ וַיֹּאמֶר יוֹנָה מִתְּחִלָּה תִּהְיֶה לִּי:</p> <p>⁵ וַיֵּצֵא יוֹנָה מִן־הָעִיר וַיֵּשֶׁב מִקְדָּם לְעִיר וַיֵּשֶׁב לוֹ שָׁם סֶכֶה וַיֵּשֶׁב</p> <p>תַּחְתֶּיהָ בְּצֵל עַד אֲשֶׁר יֵרָא מִן־הָעִיר כִּעִיר: ⁶ וְהָיָה אֲלֵהֶם</p> <p>קִקְלָו וַיֵּשֶׁב מִשָּׁל לַיּוֹנָה לִהְיוֹת צֵל עַל־רִאשׁוֹ לְהַצִּיל לּוֹ מִמָּוֶתוֹ</p>

The gradual shift to the right of both the second and third lines is more prominent in the modern editions, and the fourth line appears farther to the left on the fragment than in the modern editions. Still, the layouts are quite similar.

(51) A photo of the fragment can be found in Timothy D. Finlay with Nathan McAleese, and Andrew J. Zimmermann, “Numbers 8:3–5 (Inv. MOTB.SCR.003173),” in *DSSFMC*, 139.

(52) Ibid, 132.

(53) Elliger and Rudolph ed., *Biblia Hebraica Stuttgartensia*, Numbers 8:3–5.

(54) A photo of the fragment can be found in Catherine McDowell and Thomas Hill, "Jonah 4:2–5 (Inv. MOTB.SCR.003171)," in *DSSFMC*, 176.

(55) Ibid, 170.

(56) Elliger and Rudolph ed., *Biblia Hebraica Stuttgartensia*, Jonah 4:2–5.

F.201 (MOTB, SCR.003175, Neh2, Neh 2:13–16) (57)*Layout correspondence: close match*

Transcription (58)	BHK, 3rd edition Nehemiah 2: 13–16 (59)
1 [וְרִים וְשֵׁן]	וְהָאֲשֵׁפֶת וְאֵלֶּי שֶׁבַר בְּחוֹמַת יְרוּשָׁלַם אֲשֶׁר־הָיָה מִפְּרוֹצִים וְשַׁעֲרֶיהָ
2 [לֶךְ וְאֵין מְקוֹם לְבַהֲמָה לְעֵבֶר]	אֶלְלוּ בָּאֵשׁ; וְאֶעֱבֹר אֶל־שַׁעַר הַיָּיִן וְאֶל־בִּרְכַּת הַמֶּלֶךְ וְאֶדְמֹקֶם
3 [שֶׁבַר בְּחוֹמָה וְאֵשׁוּב וְאֵבֹו]	לְבִרְכַּת הַמֶּלֶךְ לְעֵבֶר מִחוּצָה; וְאֵלֶּי עָלָה בְּנֹחַל לִילָה וְאֵלֶּי שֶׁבַר בְּחוֹמָה
4 [וְנָה הַלְכָתָּו]	וְאֵשׁוּב וְאֵבֹו; בְּשַׁעַר הַיָּיִן וְאֵשׁוּב; לֹא יָדָעוּ אֲנִי הַלְכָתִי וְאֵבֹו אֲנִי עָשָׂה וְלִדְוֹרִים וְלִכְנָנִים וְלַחֲרִים וְלִסְגָּנִים וְלִילָה

The only obvious difference between BHK and the fragment is that the text is again “split” in the modern editions, as illustrated above. Had the page been cut in half and glued together by its outer edges, it would be plain to see that the line to line layout of the fragment is virtually identical to BHK.

Notable variants

The first three legible letters on the fragment, רים, do not correspond to any known textual witnesses. The critical apparatus informs of a *Ketiv/Qere* tradition in \mathfrak{M} , *Ketiv* reading המפורצים and *Qere* reading הם פרוצים. This is a reading which is considered “a long-standing textual mystery in MT.” (60) It is possible that this variant was created in order to excite scholars who have been hoping for a solution to the difficult reading of \mathfrak{M} in v. 13.

Furthermore, although the editors’ transcription of the text renders the end of line 3 as a *waw*, it much closer resembles “an annotation – a superscripted Greek letter α – that appears in the printed text of Kittel’s third edition of *Biblia Hebraica*.” (61) This surely is a very suspicious variant.

(57) A photo of the fragment can be found in Martin G. Abegg Jr. with Ryan Blackwelder, Joshua A. Matson, Ryan D. Schroeder, and Joseph Kyle Stewart, “Nehemiah 2:13–16 (Inv. MOTB.SCR.003175),” in *DSSFC*, 220.

(58) *Ibid*, 213.

(59) The text starts on the bottom of p. 1302 and continues at the top of p. 1303. For the sake of the illustration, the bottom of p. 1302 has been attached to the top of p. 1303. See Rudolf Kittel ed., *Biblia Hebraica*, 3rd edition (Stuttgart: Privilegierte Württembergische Bibelanstalt, 1937), Nehemiah 2:13–16.

(60) Abegg Jr. et al., “Nehemiah 2:13–16,” 216.

(61) Kipp Davis, “Paleographical and Physical Features of the Dead Sea Scrolls in the Museum of the Bible Collection: A synopsis,” in *DSSFC*, 27. See also Davis, “Caves of Dispute,” 260–261.

Summary of findings for fragments known to be modern forgeries

Six of the ten fragments show textual correspondence to modern editions of the Hebrew Bible. These six fragments are F.103 (Exod), F.112 (1 Sam), F.122 (Neh), F.194 (Num), F.197 (Jon), and F.201 (Neh). Five exhibit a close match in layout and one corresponds by a consistent shift. Two fragments contain notable variants. Of the six fragments which confirm the hypothesis, three belong to The Schøyen Collection and three are part of the MOTB Collection.

In short, more than half of the fragments known to be modern forgeries confirm the hypothesis of textual correspondence. This indicates that textual correspondence between the fragments and modern editions of the Hebrew Bible is in some cases a characteristic of modern forgery.

The analysis also illustrates, however, that some modern forgeries do not correspond to modern editions. One possible explanation is that some forgers have been more sophisticated when producing fragments, for example making sure that the layout will not match that of the edition they are copying from. Other fragments may have been copied from other sources, such as DJD.

ANALYSIS OF FRAGMENTS ABOUT WHICH CONCERNS WERE RAISED IN *GLEANINGS* AND *DSSFC*

There is a total of six fragments about which concerns were raised in the official publications of the MOTB Collection (F.195, F.198, and F.200) and The Schøyen Collection (F.113, F.116, and F.119), all of which were acquired by their current owners in 2009 or 2010. (62)

F.116 (Schøyen, MS4612/9, Jer1, Jer 3:15–19) (63)

Notable variants

רעה (*shepherd*) differs from מ but corresponds to the critical apparatus' suggested translation of ט. (6464) The fragment does not, however, exhibit the added אי (*how*) prior to ארון (*Ark of Covenant*), as suggested by the editors of BHS. (65)

(62) Justnes and Kjeldsberg, "The Post-2002 Dead Sea Scrolls Fragments."

(63) A photo of the fragment can be found in Michael Langlois, "Paleographical Analysis of the Dead Sea Scrolls in the Schøyen Collection," in *Gleanings*, 87. The transcription is found in Torleif Elgvin and Kipp Davis, "MS 4612/9. 4Q(?)Jer (Jer 3.15–19)," in *Gleanings*, 215.

(64) The graphic similarities between *resh* and *dalet* may, of course, lead to misinterpretation. It is therefore possible that the fragment corresponds to מ which reads דעה.

(65) Elgvin and Davis, "MS 4612/9. 4Q(?)Jer (Jer 3.15–19)," in *Gleanings*, 219.

The most interesting variant on the fragment is **אמן יהיה כי** (*let it be so, Lord*) on the sixth line (Jer 3:19) which differs from **ו**, but corresponds to a suggested translation from **ט** found in the critical apparatus of BHS. It is quite extraordinary that the fragment matches the suggested translation completely.

F.198 (MOTB, SCR.003183, Mic1, Mic 1:4–6) (66)

Notable variants

וּבַחֲטוֹת does not correspond with any known textual witnesses and may be a misspelling of either **וּבַחֲטָאוֹת** (*and in sins*), or the Hebrew equivalent of the singular form in **ט**, **καὶ διὰ ἁμαρτίαν** (*and through sin*). Furthermore, **בֵּית יְהוּדָה** (*house of Judah*) differs from **ו** but corresponds with a suggested reading offered by the editors of BHS, making this a notable variant.

In three instances where the apparatus lists variant readings, the fragment corresponds with **ו**: **מִי** (*who, whom*) on line two differs from **וּמָה** (*and what*) in a DSS manuscript. **בְּמוֹת יְהוּדָה הַלּוֹא יְרוּשָׁלַם** (*the high place of Judah? Is it not Jerusalem?*) differs from the editors of BHS's suggestion that this may be a later addition. Lastly, the fragment corresponds with **ו** in reading **בְּמוֹת** (*high place*), and thus differs from **ט** which reads **ἡ ἁμαρτία** (*the sin*).

F.200 (MOTB, SCR.003170, Dan6, Dan 10:18–20) (67)

Layout correspondence: close match

Transcription (68)	BHK, Daniel 10:18–20 (69)
1 [כִּמְרָן הָהוּא]	וְאֵנִי מַעֲתִילָה לֹא־יַעֲמִד־בִּי כָח וְנִשְׁמָה לֹא נָשָׁא
2 [וְהִתְחַזַּק וּכְדַבְּרוּ עִמִּי הֵתָ]	וְיִנְעִי בְּמִרְאֵה אָדָם וְיִחַזְקֵנִי: ¹⁹ וַיֵּאמֶר אֱלֹהִים
3 [הִידַעַת לְמָה בָּאתִי אָ]	שְׁלוֹם לָךְ חֲזַק וְחֲזַק וּכְנַפְרֵךְ עִמִּי הִתְחַזְקִיתִי נֶאֱמַר חֲזַקְתִּי: ²⁰ וַיֵּאמֶר הִידַעַת לְמָה־בָּאתִי אֵלֶיךָ וְעַתָּה עִם־שֵׁר פָּרַס וְאֵנִי יוֹצֵא וְהִנֵּה שָׂרִיגִין בָּאִים: ²¹ וְהָיָה הָרָשׁוּם בְּכֶתֶב אֱמֶת וְאִין אֶחָד מִתְחַזֵּק עִמִּי עַל־אֵ

(66) A photo of the fragment can be found in Peter W. Flint and David R. Herbison, "Micah 1:4–6 (Inv. MOTB.SCR.003183)," in *DSSFCM*, 189. The transcription is found on p. 179.

(67) A photo of the fragment can be found in Robert Duke with Daniel Holt and Skyler Russel, "Daniel 10:18–20 (Inv. MOTB.SCR.003170)," in *DSSFCM*, 208.

(68) Ibid., 203.

(69) Kittel ed., *Biblia Hebraica*, Daniel 10:18–20.

Notable variants

וַתַּחֲזֹק (*and make yourself strong*) differs from מַ. However, the critical apparatus provides two suggestions, based on a few 6 manuscripts. The second of these suggestions is a complete match with the reading on the fragment.

Summary of findings for fragments about which concerns were raised

Three of the six fragments show textual correspondence to modern editions of the Hebrew Bible. These are F.116 (Jer), F.198 (Mic), and F.200 (Dan). One exhibits a close match in layout, and all three contain notable variants. One of the fragments belongs to The Schøyen Collection and the remaining two are part of the MOTB Collection.

ANALYSIS OF REMAINING FRAGMENTS

There is a total of eleven remaining post-2002 fragments in The MOTB Collection (F.192 [Exod], F.196 [Ezek], and F.199 [Ps]) and The Schøyen Collection (F.101 [Gen], F.107 [Num], F.108 [Deut], F.109 [Deut], F.114 [2 Sam], F.115 [1 Kgs], F.118 [Ps], and F.120 [Ruth]), three of which were purchased by their current owners in 2003 or 2004, (70) and eight of which were acquired by their current owners in 2009 or 2010. (71)

F.108 (Schøyen, MS5214/1, Deut5, Deut 6:1–2) (72)

Layout correspondence: close match

Transcription (73)

BHK, Deuteronomy 6:1–2 (74)

1 [וְהַמְצוּהָ וְהַחֻקִּים וְ]

2 [וְעֲשׂוֹת בְּאֵרֶץ שׁוֹ]

3 [לְ]

וְזֹאת הַמְצוּהָ הַחֻקִּים וְהַמְשָׁפְטִים אֲלֵי
אֲתֶכֶם לַעֲשׂוֹת בְּאֶרֶץ אֲשֶׁר אַתֶּם
תֵּיכָא אֶת־יְהוָה אֱלֹהֵיךָ לְשֹׁמֵר אֶת־כָּל

(70) F.108 (Deut), F.114 (2 Sam), and F.118 (Ps).

(71) F.101 (Gen), F.107 (Num), F.109 (Deut), F.115 (1 Kgs), F.120 (Ruth), F.192 (Exod), F.196 (Ezek), and F.199 (Ps). Justnes and Kjeldsberg, "The Post-2002 Dead Sea Scrolls Fragments."

(72) A photo of the fragment can be found in Torleif Elgvin, "MS 5214/1. 4Q(?) Deut1 (Deut 6.1–2)," in *Gleanings*, 173.

(73) Ibid.

(74) Kittel ed., *Biblia Hebraica*, Deuteronomy 6:1–2.

The first two lines on the fragment follow the line to line layout of BHK and BHS very closely. According to the reconstruction found in *Gleanings*, the third line does not correspond as well to the layout. However, as only the top of the lamed is visible on this line, one cannot know for sure that the reconstruction renders a correct image of which *lamed* this should represent. In the reconstruction in *Gleanings*, it is suggested that it represents the *lamed* in *לשמר* (*by keeping*). Another possibility is that it represents the *lamed* of *אלהיך* (*your God*). It is surprising that this suggestion is not mentioned in *Gleanings*, as such a reconstruction would result in a line to line layout which is almost identical to that in BHK. There is no reason not to consider this possibility, and indeed it is favored in this analysis.

F.196 (MOTB, SCR.003174, Ezek1, Ezek 28:22) (75)

Layout correspondence: close match

Transcription (76)	BHK, Ezekiel 28:22 (77)
1 ואמרת כה אמר הנני עליכה צ'	22 ואמרת כה אמר אדני יהוה
2 יהוה בעשותי [] ה שפטים ונקדש	הנני עליך צידון ונקדתי בתוכך פיראני יהוה בעשותי כה שפטים ונקדשתי כה: 23 ושלחתי כה דבר ודם בתוצאותיה

As is illustrated above, the line to line layout on the fragment is not identical to the modern editions. However, the words *אדני יהוה* (*Lord YHWH*) are absent on the fragment, with the result that *הנני עליכה צ'* follows right after *ואמרת כה אמר*. In light of this, the layouts match remarkably well.

Summary of findings for remaining fragments

Two of the eleven fragments show textual correspondence to modern editions of the Hebrew Bible. These are F.108 (Deut) and F.196 (Ezek). Both exhibit a close match in layout, but none contain notable variants. One of these fragments belongs to The Schøyen Collection and the other is part of the MOTB Collection.

(75) A photo of the fragment can be found in Ishwaran Mudliar, "Ezekiel 28:22 (Inv. MOTB.SCR003174)," in *DSSFC*, 166.

(76) *Ibid.*, 161.

(77) The verse starts on the bottom of p. 790 and continues at the top of p. 791. For the sake of the illustration, the bottom of p. 790 has been attached to the top of p. 791. See Kittel ed., *Biblia Hebraica*, Ezekiel 28:22.

SUMMARY AND CONCLUSION

As is evident, half of the fragments about which concerns were raised match the criteria set forth in this analysis, while only two of the eleven remaining fragments do the same. This may seem a meager gain. Yet, for the biblical passages represented on those two fragments, the results may be of importance. More importantly, however, this analysis provides evidence that some forgers were very reliant upon modern critical editions of the Hebrew Bible in their fragment production, and that they in turn failed to make sufficient changes to the texts not to raise suspicions. In this respect, the results of this analysis are of great importance.

The results will now be presented separately for each collection because textual correspondence seems to be disproportionately present in MOTB fragments compared to Schøyen fragments.

Schøyen

Sixteen fragments in this analysis belong to The Schøyen Collection. Six of these were purchased in 2003/2004, (78) and the remaining ten were acquired in 2009/2010. (79) All of them were sold to Schøyen by William Kando, one (F.122 [Neh]) probably via Biondi and Greatsite.com. (80)

Three of the sixteen Schøyen fragments exhibit a close match in layout to the modern editions of the Hebrew Bible or DJD, (81) and one corresponds by a consistent shift. (82) Two fragments contain notable variants. (83) This means that five of the sixteen Schøyen fragments show textual correspondence to modern editions of the Hebrew Bible. These five fragments are F.103 (Exod), F.108 (Deut), F.112 (1 Sam), F.116 (Jer), and F.122 (Neh). Two of them were purchased in 2003 or 2004, (84) while the remaining three were acquired in 2009. (85) Of these five fragments, three have already been identified as modern forgeries and concerns were voiced about one in *Gleanings*.

(78) F.103 (Exod), F.104 (Exod), F.105 (Exod), F.108 (Deut), F.114 (2 Sam), and F.118 (Ps).

(79) F.101 (Gen), F.107 (Num), F.109 (Deut), F.112 (1 Sam), F.113 (1 Sam), F.115 (1 Kgs), F.116 (Jer), F.119 (Prov), F.120 (Ruth), and F.122 (Neh).

(80) Justnes and Kjeldsberg, "The Post-2002 Dead Sea Scrolls Fragments."

(81) F.103 (Exod), F.108 (Deut) and F.122 (Neh). Note that only F.103 (Exod) was compared to DJD. In a subsequent analysis, this should be done for all post-2002 fragments.

(82) F.112 (1 Sam).

(83) F.116 (Jer) and F.122 (Neh).

(84) F.103 (Exod) and F.108 (Deut).

(85) F.112 (1 Sam), F.116 (Jer), and F.122 (Neh). Ibid.

Museum of the Bible

Eleven fragments addressed in this analysis are part of the MOTB Collection. Ten of these were purchased by their current owners in 2009/2010, and the last was acquired in 2014 or 2015. (86) Seven fragments were allegedly sold to Steve Green by William Kando, (87) one was sold by Michael Sharpe, (88) and one by Andrew Stimer. (89) The last two were sold to Green from Craig Lampe. (90)

Five of the eleven MOTB fragments exhibit a close match in layout to the modern editions of the Hebrew Bible, (91) and three fragments contain notable variants. (92) In total, six of the eleven MOTB fragments show textual correspondence to modern editions of the Hebrew Bible. These six fragments are F.194 (Num), F.196 (Ezek), F.197 (Jon), F.198 (Mic), F.200 (Dan), and F.201 (Neh). Of these fragments, three have already been identified as modern forgeries and concerns were voiced about two in *DSSFC*.

Interestingly, all six fragments which show signs of textual correspondence were purchased in 2010 from William Kando. In other words, six of the seven fragments purchased directly from William Kando show signs of textual correspondence. The two fragments acquired from Lampe in 2009 do not exhibit this feature. Nor do the two last fragments which were obtained by the Green family in 2010 and 2014 or 2015. In short, the only three fragments which do not show any textual correspondence, and which were not known to be modern forgeries at the time this analysis was conducted, are F.195 (Jer) and the two fragments acquired from Lampe in 2009: F.192 (Exod) and F.199 (Ps).

The proportion of MOTB fragments which show a textual correspondence to modern editions of the Hebrew Bible is considerably higher than that of The Schøyen Collection (although the significance of the five Schøyen fragments should not be underestimated). (93) A different way of presenting the results is by year of acquisition. Textual correspondence to modern editions occurs more often in fragments acquired in 2009 or later than in those acquired before 2009. (94) One

(86) F.203 (Lev). Ibid.

(87) F.194 (Num), F.195 (Jer), F.196 (Ezek), F.197 (Jon), F.198 (Mic), F.200 (Dan), and F.201 (Neh).

(88) F.191 (Gen).

(89) F.203 (Lev).

(90) F.192 (Exod) and F.199 (Ps). Ibid.

(91) F.194 (Num), F.196 (Ezek), F.197 (Jon), F.200 (Dan), and F.201 (Neh).

(92) F.198 (Mic), F.200 (Dan), and F.201 (Neh).

(93) 54,5 % in MOTB fragments and 31,2 % in Schøyen fragments.

(94) 33,3 % of fragments acquired prior to 2009 and 42,9 % of fragments acquired in 2009 or later.

possible explanation is that there are several (groups of) forgers who utilize different techniques, and that one (or more) of these are quite banal in their use of modern editions of the Hebrew Bible in their fragment production. It seems that they copy the text from modern editions of the Hebrew Bible without making any emendations to the layout, whilst often importing variant readings from the critical apparatus. In these cases, the layout most often (though not always) appears to be influenced by BHK, while the variants seem to be imported from the critical apparatus in BHS.

APPENDIX

F. no	Collection	Collection no.	F. Name	Content
101	Schøyen	4612/4	Gen1	Gen 36:7–16
103	Schøyen	4612/2a	Exod3	Exod 3:13/14–15
104	Schøyen	4612/2b	Exod4	Exod 5:9–14
105	Schøyen	4612/2c	Exod5	Exod 16:10
107	Schøyen	4612/5	Num1	Num 16:2–5
108	Schøyen	5214/1	Deut5	Deut 6:1–2
109	Schøyen	5214/2	Deut6	Deut 32:5–9
112	Schøyen	4612/10	Sam1	1 Sam 2:11–14
113	Schøyen	5480/4	Sam2	1 Sam 5:10–11
114	Schøyen	5233/1	Sam3	2 Sam 20:22–24
115	Schøyen	5440	Kgs1	1 Kgs 16:23–26
116	Schøyen	4612/9	Jer1	Jer 3:15–19
118	Schøyen	5233/2	Ps2	Ps 9:10, 12–13
119	Schøyen	4612/11	Prov1	Prov 4:23–5:1
120	Schøyen	5441	Ruth1	Ruth 2:1–2
122	Schøyen	5426	Neh1	Neh 3:14–15
191	MOTB	SCR.000124	Gen2	Gen 31:23–25(?) and 32:3–6
192	MOTB	SCR.000120	Exod6	Exod 17:4–7
194	MOTB	SCR.003173	Num2	Num 8:3–5
195	MOTB	SCR.003172	Jer2	Jer 23:6–9
196	MOTB	SCR.003174	Ezek1	Ezek 28:22
197	MOTB	SCR.003171	Jon1	Jon 4:2–5

F. no	Collection	Collection no.	F. Name	Content
198	MOTB	SCR.003183	Mic1	Mic 1:4–6
199	MOTB	SCR.000121	Ps3	Ps 11:1–4
200	MOTB	SCR.003170	Dan6	Dan 10:18–20
201	MOTB	SCR.003175	Neh2	Neh 2:13–16
203	MOTB	SCR.004742	Lev6	Lev 23:24–28

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THE SONS OF ZADOK, THE HASMONEAN ROYAL HIGH PRIESTHOOD, AND THE BOOK OF EZEKIEL

Summary

The re-emergence of the Sons of Zadok in CD-A, and later in D^a, 1QS–1QSa–1QSB, 4QFlor, and 4Qpap pIsa^c has generated a variety of scholarly interpretations. Many scholars have looked at the Sons of Zadok mainly in relation to the Sons of Aaron, and most often within the setting of the *yahad*. While some argued for an exile of the Zadokites from Jerusalem, many other scholars posited internal fractures within the *yahad* itself; still others proposed this appellation should not be understood in genealogical sense or even that it should be taken as a literary invent. First, this study proposes to look at the Sons of Zadok against the backdrop of Hasmonean rule. It does this by interpreting this designation in the light of Ezek 40–48 and in co-relation with other ideologies that had been imported into the DSS. As in Ezek 40–48, in the DSS the Sons of Zadok are used ideologically. The adoption of this appellation was an ideological strategy by the Zadokite scribes to further validate their two Messiah eschatology, which stipulates a separation of the high priesthood from royal office. It appears to have implied refusal to accept the authority of the Hasmonean ruler, Alexander Janneus, especially as high priest. By the same token, this designation reflects a further radicalization and self-perception of the *yahad* itself, or of significant part of it. Second, this study suggests that all these DSS derive from the same circle of scribes. Even though all, or almost all, are probably based on earlier exemplars, these scrolls appear to have been produced to be read together. The re-emergence of the Sons of Zadok in the DSS as well as probably in Sir 51:12a–o can thus tentatively be dated to the time of Janneus. Political and other changes following his death may be an explanation why the Sons of Zadok do not appear in later DSS.

0. Background

OUTSIDE the Hebrew Bible, the “Sons of Zadok” (בני זדוק) first came to the attention of modern scholars through two medieval Hebrew exemplars of the composition Solomon Schechter designated as the “Zadokite Work”. These manuscripts were discovered in 1897 in the Genizah of the Ben Ezra Synagogue in Cairo: the much longer MS A dates from the 10th c., while MS B dates from the 12th. Later this composition became known as the Cairo Damascus Document (CD). (1) The Sons of Zadok were also found in Sir 51:12a–o (MS B), a Hebrew text originating from the same Genizah. (2) Some fifty-five years later, the Sons of Zadok came to light in seven non-biblical Dead Sea Scrolls (=DSS), occurring altogether probably twelve times. They appear three times in the Damascus Document: twice in CD 3:20–4:4 (MS A; once as part of a quotation of Ezekiel 44:15 followed by an interpretation) and once in 4QD^a (4Q266) 5 I 16//4Q267 5 II [?]¹—a text similar to the interpretation in CD 4. (3) The designation also occurs two

* I thank Chris Seeman for improving the English of this essay and for making some valuable suggestions. Any remaining errors are my sole responsibility. This study is based on two separate conference papers delivered at *The Fifth Groningen–Leuven Encounter on the Dead Sea Scrolls: Dead Sea Scrolls and Hellenism*, organized by the Qumran Institute in Groningen, Netherlands, in 2016, and at the *SBL International Meeting* in Helsinki, Finland, in 2018.

Solomon Schechter, *Documents of Jewish Sectaries, Vol. I: Fragments of a Zadokite Work. Prolegomenon* by Joseph A. Fitzmyer (ed. Harry M. Orlinsky; Cambridge: 1910. Repr. New York, NY: Ktav, 1970). For a later and more complete edition of CD, see Joseph M. Baumgarten and Daniel R. Schwartz; with an appendix by Y. Ofer, “The Damascus Document (CD),” in *The Dead Sea Scrolls: Hebrew, Aramaic, and Greek Texts with English Translations: Damascus Document, War Scroll, and Related Documents, Vol 2* (eds. James H. Charlesworth et al.; The Princeton Theological Seminary Dead Sea Scrolls Project; Tübingen: Mohr Siebeck; Louisville, KY: Westminster/John Knox, 1995), 4–57 [= DSSHAGT 2].

(1) The term ‘exemplar/s’ used here does not mean an equal ‘copy’ of a given composition. Sometimes it may imply distinguishable literary editions or version/s, which share some common material. Even such terms as ‘literary edition/s’ or ‘version/s’ is often debated as there are hardly any criteria of ‘how much’ common material (and structure) another scroll should share for identifying it another ‘version’. Likewise, there is the problem of textual dependence as, in principle, no ‘version’ can be considered *a priori* a source-text for the other.

(2) For the text of Sir 51:12a–o, see Pancratius C. Beentjes, *The Book of Ben Sira in Hebrew: A Text Edition of All Extant Hebrew Manuscripts and a Synopsis of All Parallel Hebrew Ben Sira Texts* (VTS 68; Leiden: Brill, 1997), 92–93. There is also one small parchment fragment mentioning the Sons of Zadok reportedly from the same Genizah, but its origins are obscure. See Israel Lévi, “Document relatif à la ‘Communauté des fils de Sadoc’,” *REJ* 65 (1913): 24–31.

(3) There are ten recognizable D exemplars found in Qumran caves. They parallel nearly half of the Cairo texts (some 326 lines, complete or partial) but also cover some

or three times in the Community Rule (*Serekh ha-Yahad*) 1QS 5:2, 9 and in 9:14—if one interprets בני הצדוק to mean “Sons of Zadok” rather than “Sons of Righteousness”. (4) It also appears three times in the Rule of the Congregation (*Serekh ha-Edah*) 1QSa (1Q28a) 1:2, 24; 2:3. (5) The so-called Rule of Benedictions 1QSb (1Q28b) 3:22–24 refers once to the Sons of Zadok as part of a possible interpretation based on Ezek 44:15, 23–24. (6) One mention is also preserved in the thematic 4Qpap pesher Isaiah^c (4Q163) 22 3, possibly an interpretation of Isa 30:1–5. (7) The same designation is contained in the non-thematic

significant ‘lacking’ material in the medieval MS A 1–16 and MS B 19–20. For editions, see Joseph M. Baumgarten; with contributions by Stephen Pfann and Ada Yardeni, eds., *Qumran Cave 4, XIII: The Damascus Document (4Q266–273). On the Basis of Transcriptions by Józef T. Milik* (DJD 18; Oxford: Clarendon Press, 1996); Joseph M. Baumgarten with Michael T. Davis, “Cave IV, V, VI Fragments Related to the Damascus Document (4Q266–273 = 4QD^{a-h}, 5Q12 = 5QD, 6Q15 = 6QD),” in DSSHAGT 2, 59–79; Hartmut Stegemann, “269. 4QDamascus Document^d frgs. 10, 11 (Re-edition), 15, 16,” in *Qumran Cave 4, XXVI: Cryptic Texts and Miscellanea, Part I* (eds. Stephen J. Pfann et al.; DJD 36; Oxford: Clarendon Press, 2000), 201–11.

(4) For the “Sons of Zadok” as the more probable reading, see Robert Kugler, “A Note on 1QS 9:14: The Sons of Righteousness or the Sons of Zadok?” *DSD* 3/3 (1996): 315–20. There are eleven or twelve exemplars traditionally identified by scholars as belonging to the S textual tradition: 1QS, 4QS^{a-j} (4Q255–264) and 5QS (5Q11). For the edition of 1QS, see Elisha Qimron and James H. Charlesworth, “Rule of the Community (1QS),” in *The Dead Sea Scrolls: Hebrew, Aramaic, and Greek Texts with English Translations: Rule of the Community and Related Documents, Vol. I* (eds. James H. Charlesworth et al.; The Princeton Theological Seminary Dead Sea Scrolls Project; Tübingen: Mohr Siebeck; Louisville, KY: Westminster/John Knox, 1994), 1–51 [= DSSHAGT 1]; Corrado Martone, *La “Regola della Comunità. Edizione Critica* (Quaderni di Henoch 8; Torino: Silvio Zamorani Editore, 1995), 13–159. For an edition of 4QS texts, see Philip S. Alexander and Geza Vermes, *Qumran Cave 4, XIX: Serekh Ha-Yahad and Two Related Texts* (DJD 26; Oxford: Clarendon Press, 1998). See recently, Sarianna Metso, Michael A. Knibb, and Chad Martin Stauber, *The Community Rule: A Critical Edition with Translation* (SBL.EJL; Atlanta, GA: SBL, 2019).

(5) For editions of 1QSa, see Dominique Barthélemy, “1Q28a. Règle de la Congrégation (1QSa),” in *Qumran Cave I* (eds. Dominique Barthélemy and Józef T. Milik; DJD 1; Oxford: Clarendon Press, 1955), 107–18; James H. Charlesworth and Loren T. Stuckenbruck, “Rule of the Congregation (1QSa),” in DSSHAGT 1, 108–17. For 4QSa fragments, see Stephen J. Pfann, “4Q249^{a-i}: 4QpapCryptA Serekh ha-Edah^{a-j},” in DJD 36, 515–74.

(6) For editions, see Józef T. Milik, “28b. Recueil des Bénédiction,” in DJD 1, 118–30; James H. Charlesworth and Loren T. Stuckenbruck, “Blessings (1QSb),” in DSSHAGT 1, 119–31. A further fragment of 1QSb V 22–25 was published by George J. Brooke, “1QS28b. 1QSerekh ha-Yahad b (fragment),” in DJD 26, 227–33.

(7) There are six known exemplars of Pesher Isaiah: 3QpIsa, 4QpIsa^{a-c}. For editions, see John M. Allegro; with the collaboration of Arnold A. Anderson, eds., *Qumran Cave 4, I (4Q158–4Q186)* (DJD 5; Oxford: Clarendon Press, 1968), 11–30; on 4QpIsa^c, see also Maurya P. Horgan, “Isaiah Pesher 3 (4Q163 = 4QpIsa^c),” in *The Dead Sea Scrolls: Hebrew, Aramaic, and Greek Texts with English Translations*:

peshar 4QFlorilegium (4Q174) 1–2 I 16–17, which seems to interpret Ezek 37:23, 44:10, or perhaps both. (8)

Some scholars have argued that “Sons of Zadok” was interpolated in some of these scrolls. Thus, whereas 1QS 5:2 refers to the authority of “the Sons of Zadok and the multitude of the people of the *yahad*”, its parallels in 4QS^b (4Q256) 5 IX 3 and 4QS^d (4Q258) 1 I 2 speak of the authority of “the Many” (הרבים). Likewise, 1QS 5:9 refers to the authority of the “Sons of Zadok, the priests and the multitude of the people of their covenant”, but its parallels in 4QS^b (4Q256) 5 IX 7–8 and 4QS^d (4Q258) 1 I 7 speak of the authority of “the Council of the people of the *yahad*”. (9) As for 1QS 9:14, it states that “the Maskil should separate and evaluate the Sons of Zadok (בני הצדוק)”. By contrast, the parallel in 4QS^c (4Q259) 1 III 10 seems to assume that “the Maskil should separate and evaluate the Sons of Righteousness (בני הצדק)”.

As detailed in the survey below, these and other significant aspects led many scholars to give considerable attention to the Sons of Zadok both from literary and historical points of view. Understanding the Sons of Zadok in the DSS is also important because this designation appears almost exclusively in scrolls where *yahad* is mentioned. (10)

Pesharim, Other Commentaries, and Related Documents, Vol. 6B (eds. James H. Charlesworth et al.; The Princeton Theological Seminary Dead Sea Scrolls Project; Tübingen: Mohr Siebeck; Louisville, KY: Westminster/John Knox, 2002), 47–81 [= DSSHAGT 6B].

(8) For editions, see John M. Allegro, “174. Florilegium,” in DJD 5, 53–57; Jacob Milgrom, “Florilegium: A Midrash on 2 Samuel and Psalms 1–2 (4Q174=4QFlor),” in DSSHAGT 6B, 248–63; Annette Steudel, *Der Midrasch zur Eschatologie aus der Qumrangemeinde (4QMdrEschat^{a,b})* (STDJ 13; Leiden: Brill, 1994). 4Q174 appeared in editions and studies under various names, for which, see George J. Brooke, “From Florilegium or Midrash to Commentary: The Problem of Re-Naming an Adopted Manuscript,” in *The Mermaid and the Partridge: Essays from the Copenhagen Conference on Revising Texts from Cave Four* (eds. George J. Brooke and Jesper Høgenhaven; STDJ 96; Leiden: Brill, 2011), 129–50.

(9) For the view of 1QS as a later ‘version’, see already Geza Vermes, “Preliminary Remarks on Unpublished Fragments of the Community Rule from Qumran Cave 4,” *JJS* 42/2 (1991): 250–55.

(10) CD (MS B) 20:1, 14, 32 has היחיד which some scholars read as היחד; so, for example, Elisha Qimron, “The Text of CDC,” in *The Damascus Document Reconsidered* (ed. Magen Broshi; Jerusalem: The Israel Exploration Society, 1992), 9–49; Schofield, *From Qumran to the Yahad: A New Paradigm of Textual Development for the Community Rule* (STDJ 77; Leiden: Brill, 2009), 172. Other scholars think that only אנשי היחיד in CD 20:32 should be translated as “men of the Community” (e.g., 4QS^b 9:8; 4QS^d 1:7); so, for example, Joseph M. Baumgarten and Daniel R. Schwartz, “Damascus Document (CD),” in DSSHAGT 2, 34–37. However, CD mentions several times עדה, which was another self-appellation used by the Community (e.g., 1QSa 1:1). The term *yahad* is not preserved in the fragmentary 4QpIsa^c but appears instead in 4QpIsa^d (4Q164) 1:2. In principle, all *pesharim* are believed to have been produced by the *yahad*.

In fact, the term *yahad* is used as a noun with more or less ‘sectarian’ connotations at least 140 times in the DSS. 1Q alone refers to *yahad* at least 60 times and the same term appears seven times in 1QSa, and three times in 1QSB. Thus, nearly half of occurrences are found in 1QS, 1QSa, and 1QSB taken together, which were written by the same scribe and may have been once part of the same scroll. (11)

1. Questions and Proposed Methodology

For the sake of convenience, I shall refer to the seven DSS which mention the Sons of Zadok as ‘Zadokite’, and so also to the scribes responsible for this designation in these MSS. (12) As the survey will demonstrate, most scholars have looked at the Sons of Zadok in these DSS mainly in relation to the Sons of Aaron, and most often within the setting of the *yahad*. Several scholars, however, have dealt with the relation of the Sons of Zadok with the temple priesthood and other issues. To be sure, no study has addressed the specific question of their relation to Hasmonean rule. Many scholars have also shown limited concern for the great influence which the Book of Ezekiel exerted on the *yahad* texts. (13) Consequently, they have interpreted the designation Sons of Zadok separately from its original context. As we shall see, the influence of Ezekiel is not limited to the use of this designation. Ezekiel’s ideological claims had inspired important *yahad* compositions before the Sons of Zadok were imported.

(11) See already Józef T. Milik, DJD 1, 107–8. So also Hartmut Stegemann, “Some Remarks to 1QSa, to 1QSB, and to Qumran Messianism,” *RevQ* 17/4 (1996): 480–87. Eibert J.C. Tigchelaar, “In Search of the Scribe of 1QS,” in *Emanuel*, 439–52, suggested the scribe of 1QS probably held a leading position and authority to interpret Scripture within the *yahad*. Emanuel Tov, *Scribal Practices and Approaches Reflected in the Texts Found in the Judean Desert* (STDJ 54; Leiden: Brill, 2004), 105 n 149, concludes that 1QS, 1QSa and 1QSB “were probably rolled up together” but not “stitched”, at least in this order.

(12) While the notion of ‘Zadokite scribes’ is more fitting, it is less so with regard to ‘Zadokite DSS’. All seven DSS are likely based on earlier source-texts. The Zadokite scribes, who were likely priests for whom the Book of Ezekiel played an important role, reworked their *Vorlagen* by adding new material—including the Sons of Zadok passages—to produce new documents.

(13) Several scholars investigated the influence of the Book of Ezekiel on some DSS, but their focus was not the priesthood. See especially George J. Brooke, “Ezekiel in Some Qumran and New Testament Texts,” in *The Madrid Qumran Congress. Proceedings of the International Congress on the Dead Sea Scrolls, Madrid 18–21 March, 1991* (eds. Julio Trebolle Barrera and Luis Vegas Montaner; STDJ 11/1; Leiden: Brill, 1992), 318–31; Florentino García Martínez, “The Interpretation of the Torah of Ezekiel in the Texts from Qumran,” in *Qumranica Minora II: Thematic Studies on the Dead Sea Scrolls by Florentino García Martínez* (ed. Eibert J.C. Tigchelaar; STDJ 64; Leiden: Brill, 2007), 1–12.

By contrast, with minor exceptions, historians of Second Temple priesthood have shown little regard for the specific use and role of the Sons of Zadok in the DSS. They equally have paid little attention to the relation of the Sons of Zadok to the ideological themes in the DSS, that is, their new literary contexts. Also, too often these scholars have overlooked the question of textual development of the Zadokite DSS and especially their historical context.

This study proposes the following historical questions: Can we more confidently identify the Sons of Zadok in the DSS? If so, why would someone have preferred to insert this self-designation in some of most important documents, especially where the Sons of Aaron already stood? Are there any clues for dating this 'Zadokite' scribal activity, at least approximately?

The aim is to situate the Sons of Zadok, and hence their scrolls, in their literary and historical contexts. This will lead us to move beyond the oft-discussed relations of the Sons of Zadok with the Sons of Aaron as members of the same "Community" (*yahad*). We shall first briefly discuss the influence the textual tradition of Ezekiel exerted on the Zadokite scribes. We shall then look at the Sons of Zadok in the context of the religious and socio-political history of Judea by attending to the Zadokite scribal perception of Hasmonean governmental model and of themselves.

2. The Sons of Zadok in the DSS: From Literary to Historical Interpretations

I shall start by briefly introducing some theories on the textual development, in particular, of 1QS in relation to 4QS MSS. This is especially relevant as seven out of twelve occurrences are found in 1QS, 1QSa, and 1QSB, penned very probably by the same scribe. Altogether, this section will better illustrate the existing problems in the interpretation of the Sons of Zadok from both literary and interpretative perspectives.

a) The Sons of Zadok in the DSS: Literary Interpretations

It is not a fitting place to provide here an exposition of various scholarly views regarding the literary development of the DSS mentioning the Sons of Zadok. There are basically four main theories than can be discerned. The first theory is that developed by Sarianna Metso. She suggests that even though 4QS^b and 4QS^d, as well as 4QS^e, are all paleographically dated to the second half of the 1st c. BCE, their

shorter texts represent ‘versions’ older than 1QS. (14) It would thus follow that the Sons of Zadok were inserted in the ‘later’ 1QS ‘version’—a view defended today by an increasing number of scholars. (15) It would also mean that the absence of the Sons of Zadok from 4QS could perhaps mean that later scribes deemed such insertions as no longer relevant.

The opposite view is that defended by Philip A. Alexander. He challenged Metso’s theory mainly on two levels. First, 4QpapS^a (4Q255), which is paleographically dated to the end of the 2nd c. BCE., attests to a version similar to that of 1QS, which instead is dated later. Second, Alexander wonders why later scribes would have decided to copy older versions rather than the more complete 1QS. He concludes that 4QS^{b,d} are more likely abridged versions of 1QS, while 4QS^e is the latest in that line. Contrary to Geza Vermes (see below), Alexander explains the absence of the Sons of Zadok in 4QS^{b,d} as possibly reflecting a “later period when Zadokite power had waned”. (16)

(14) For arguments, see further, *Textual Development of the Qumran Community Rule* (STDJ 21; Leiden: Brill, 1997), esp. 74–90, 143–49. Metso occasionally provides different readings and even numbering of individual fragments than those found in DSSHAGT 1 and DJD 26 (see here n. 4). See also Metso, “The Redaction of the Community Rule,” in *The Dead Sea Scrolls: Fifty Years after Their Discovery 1947–1997. Proceedings of the Jerusalem Congress, July 20–25, 1997* (eds. Lawrence H. Schiffman, Emanuel Tov, and James C. VanderKam; Jerusalem: Israel Exploration Society & The Shrine of the Book, 2000), 377–84. 4QS^d was also subjected to C-14 testing. The results support Frank M. Cross’s identification of the script as “Herodian”.

(15) See, among others, Yonder M. Gillihan, *Civic Ideology, Organization, and Law in the Rule Scrolls: A Comparative Study of the Covenanters’ Sect and Contemporary Voluntary Associations in Political Context* (STDJ 97; Leiden: Brill, 2012), 310 (with some variation). See especially Charlotte Hempel, “The Long Text of the *Serekh* as Crisis Literature,” *RevQ* 27/1 (2015): 8–16 (with previous literature).

(16) “The Redaction-History of *Serekh ha-Yahad*: A Proposal,” *RevQ* 17/4 (1996): 437–53. See earlier, Alexander and Vermes, DJD 26, 27–38; similarly, Marcus Bockmuehl, “Redaction and Ideology in the Rule of the Community,” *RevQ* 18/4 (1998): 541–60. This same position was adopted by Dongshin Don Chang, *Phinehas, the Sons of Zadok, and Melchizedek: Priestly Covenant in Late Second Temple Texts* (LSTS 90; London: Bloomsbury T&T Clark, 2016), Part 2. Although Alexander’s argumentation works well when considering only the paleographic dates—which, in any case, are not conclusive—there are problems with his reconstruction on a redaction-critical level. As for 4QpapS^a, only two clearly identifiable small fragments survive for comparison with 1QS. As Alexander himself observes, the “4QSpapS^a fragments preserve some minor variants from the 1QS text. More significantly, 4QpapS^a seems to have contained material which is not found in 1QS text”. Metso, “The Redaction of the Community Rule,” 377–84, draws attention to, among others, some biblical DSS paleographically dated later but which reflect a shorter and perhaps earlier version of their composition: e.g., 4QJer^b (shorter) vs. 4QJer^a, 4QpaleoExod^m (shorter) vs. 4QExod^{b,c}. Similar scribal practice as suggested by Metso existed for the Epic of Gilgamesh. The Babylonian scribes

The third, middle theory, was developed by Alison Schofield. She opines a “semi-independent development” of 4QS^{b,d}, of 4QS^c, and 1QS, “with no one manuscript preserved for us clearly having been derived from another”, or within the same scribal community. For Schofield, “1QS may have been the official Qumran copy”, which she sees as the center of the *yahad*, but its redactors did not use 4QS^{b,d} (as we now know them). Schofield thinks the Sons of Zadok are absent from some ‘S’ versions probably because they were not present in every group that produced these scrolls. (17)

The fourth view is that formulated by Jutta Jokiranta and Hanna Vanonen. Their point of departure is that 4QS MSS should be first evaluated on their own right. They point out how a MS like 5Q13 may display closer affinities with 1QS, than, for example, certain fragments traditionally assigned by scholars to 4QS^c, 4QpapS^a, or 4QS^b. They believe that the shared material between the MSS should be regarded as prototypical. To this ‘Urtext’, scribes had been adding new material which resulted in new versions—hence, not a linear development—parts of which are probably lost to us. As for “S”, it is an “abstract” category which only exists “in our minds”. Therefore, the search for the textual development of the ‘S’-tradition is far more complex than previously assumed. (18)

not only continued to preserve Old (2000–1600) and Middle (1600–1000) Babylonian versions of the epic, but occasionally copied them. This despite the fact that the Late Babylonian version (1000–700 BCE) became the standard ‘canonical’ text. See Andrew R. George, *The Babylonian Gilgamesh Epic: Introduction, Critical Edition and Cuneiform Texts, Vol. I* (Oxford: University Press, 2003), 39, 352–53, 369–70, 380, 391. By contrast, later Babylonian copyists would sometimes also omit whole lines or phrases, and abbreviate or rewrite other portions of their base-text. However, major changes are noted in the Epic’s narrative material. See John Tigay, *The Evolution of the Gilgamesh Epic* (Philadelphia: University of Pennsylvania Press, 1982), 62–63, 71–73, 91–93. On historical epitomes in Greco-Roman world (e.g., 2 Macc 3–15), see Robert A. Derrenbacker Jr., *Ancient Compositional Practices and the Synoptic Problem* (BETHL 186; Leuven: Peeters, 2005), 66–69. On how the epitomes supplanted their sources, see Peter A. Brunt, “On Historical Fragments and Epitomes,” *CQ* 30/2 (1980): 477–94.

(17) *From Qumran to the Yahad*, 7, 128, 130, 147–52. Schofield hypothesizes that 4QS^c may have originated from a branch of the *yahad* located in Jerusalem. See also the positive review on the monograph of Schofield by John J. Collins in *DSD* 17/1 (2010): 131–33.

(18) For details, see “Multiple Copies of Rule Texts or Multiple Rule Texts? Boundaries of the S and M Documents,” in *Crossing Imaginary Boundaries: The Dead Sea Scrolls in the Context of Second Temple Judaism* (eds. Mika S. Pajunen and Hanna Tervanotko; Publications of the Finnish Exegetical Society 108; Helsinki: The Finnish Exegetical Society, 2015), 11–60 19–22, 28–34, 38–55. 19–22, 28–34, 38–55. See also Devorah Dimant, *History, Ideology and Bible Interpretation in the Dead Sea Scrolls: Collected Studies* (FAT 90; Tübingen: Mohr Siebeck, 2014), 173–77.

True, one needs to further prove that all ‘S’ exemplars were, in fact, produced within the same *yahad*, or within the same branch of the *yahad*. Besides, 4QS^e is characterized by elements not easily reconcilable with either ‘S’ MSS. Because some significant difficulties persist, the theory of Schofield on the one hand, and that of Jokiranta and Vanonen on the other, all of whom did not reject Metso’s conclusions altogether, deserve closer attention. (19) It recalls biblical compositions that had circulated for a period in two or more versions: e.g., Exodus, Jeremiah, Ezekiel, and many others. There is no reason why the scribes behind the Zadokite MSS would have behaved very differently from those responsible for the many biblical MSS. Nor is there any certainty that all MSS related to the ‘S’ textual tradition have been preserved, just as there is no certainty that a ‘corrected’ exemplar of 1QS ever existed. Similarly, one can theorize that there may once have been more fragments, or even MSS, which mentioned the Sons of Zadok. What especially matters here, is, that the textual ‘framework’ of the designation Sons of Zadok in 1QS is shared in 4QS^b, 4QS^d, and 4QS^e. By contrast, the designation itself is lacking, which speaks in favor of its being inserted only in 1QS.

No substantial parallels survive for other Zadokite DSS to allow for a discussion about their literary development. For example, Charlotte Hempel thinks 1QSa 1:1–3 and 1:6–2:11a “underwent a ‘Zadokite recension’ which resulted in the addition of a number of redactional passages that reflect the social background of 1QS 5,” the end-result of which was “to enhance the authority of the sons of Zadok”. (20) Hempel also claims that, “whereas the sons of Aaron are a quiet presence in several ‘S’ manuscripts as well as frequently in D, the sons of Zadok are presented as the crowning moment in the chronological development of the Community in CD 4 and in the developing authority structure portrayed in 1QS”. (21) According to Stephen Hultgren, the whole section of CD 3:17b–4:12a—which refers twice to the Sons of Zadok—belongs to a “Qumran redaction”, which he dates to the last quarter of the 2nd c. BCE, or later. (22) These MSS are further discussed in the second part of this study.

(19) Schofield, *From Qumran to the Yahad*, 130; Jokiranta and Vanonen, “Multiple Copies?” 39–40, 42–43 n. 103.

(20) *The Qumran Rule Texts in Context: Collected Studies* (TSAJ 154; Tübingen: Mohr Siebeck, 2013), 49–52, 62.

(21) *Qumran Rule Texts*, 208–209, 222, 227.

(22) *From the Damascus Covenant to the Covenant of the Community* (STDJ 66; Leiden: Brill, 2007), 506–7, 542.

b) *The Sons of Zadok in the DSS: Historical Interpretations*

As a rule, the Sons of Zadok in the DSS were approached from the perspective of their supposed relation to the Sons of Aaron. Indeed, in some DSS the Sons of Zadok are found alongside the “Sons of Aaron” (בני אהרן: 4QD^a 5 II; 1QS 5:21; 9:7; 1QSa 1:23; 2:13), or “Aaron” alone (CD; 1QS; 4QFlor 5 ii 2). (23) Apart from Ben Sira 50 and 51:12a–o (MS B), there is no other known literary composition where the Sons of Zadok appear together with the Sons of Aaron. Unlike with the Sons of Aaron, the Sons of Zadok are mentioned almost exclusively in MSS that also refer to the *yahad*.

Geza Vermes argued that both appellations are synonymous and that around the mid-2nd c. BCE a “primitive community ... was subsequently joined by a group of Zadokite priests, who achieved a successful ‘takeover’ thanks to their doctrinal expertise and social status”. (24) By contrast, Lawrence H. Schiffman thinks the “Zadokite priests” founded the “sect” after their withdrawal from the temple as a result of halakhic disagreements with the Hasmonean high priest Jonathan (152–143 BCE). (25) Charlotte Hempel posits that the Sons of Zadok and the Sons of Aaron “do not appear to be employed entirely randomly or synonymously”. A “sizeable group of texts speak of the sons of Aaron in a non-community-specific, national context”. This is especially true in the case of D, where the “designation ‘sons of Zadok’ is not used

(23) By itself, “Zadok” occurs three times in two separate scrolls: CD 5:2 and the Copper Scroll (3Q15) 11:3, 6. In all DSS, “Aaron” is mentioned some 76 times in Hebrew and nine times in Aramaic texts, while “Sons of Aaron” occurs about 30 times. See further Charlotte Hempel, “אהרן,” *ThWQ* 1:76–81; Ulrich Dahmen, “צדוק,” *ThWQ* 3:379–83.

(24) “The Leadership of the Qumran Community: Sons of Zadok—Priests—Congregation,” in *Geschichte—Tradition—Reflexion: Festschrift für Martin Hengel zum 70. Geburtstag, Vol. 1: Judentum* (eds. Hubert Cancik, Hermann Lichtenberger, and Peter Schäfer; Tübingen: Mohr Siebeck, 1996), esp. 380–81. In line with other scholars, Livia Capponi, *Il tempio di Leontopoli in Egitto: Identità politica e religiosa dei Giudei di Onia (c. 150 a.C.–73 d.C.)* (Pubblicazioni della Facoltà di Lettere e Filosofia dell’Università di Pavia 118; Pisa: Edizioni ETS, 2007), 164, *passim*, believes Onias IV, the son of Onias III—of Zadokite high priestly lineage, founded the temple in Leontopolis. By contrast, Meron M. Piotrkowski, *Priests in Exile: The History of the Temple of Onias and Its Community in the Hellenistic Period* (Studia Judaica. FWJ 106; Berlin: de Gruyter, 2019), 100–102, *passim*, favors the ‘Zadokite’ Onias III. Piotrkowski, 395, affirms to “have found no basis for the presumption that the founder of the Qumran community, the legendary ‘Teacher of Righteousness,’ was an Oniad (high) priest.” Earlier, Étienne Nodet, *La crise maccabéenne: Historiographie juive et traditions bibliques* (Joséphé et son temps 5; Paris: Cerf, 2005), 272–89, argued the Oniad dynasty had origins in Egypt, where they returned.

(25) *Qumran and Jerusalem: Studies in the Dead Sea Scrolls and the History of Judaism* (Grand Rapids, MI/Cambridge, UK: W.B. Eerdmans, 2010), 101–104.

to refer to the priests ..., but apparently refers to the community as a whole”—which is never the case with the Sons of Aaron. As for 1QS and 1QSa, Hempel thinks the Sons of Zadok appear there as authorities within the *yaḥad*, just as probably in 1QSB. (26) For his part, Heinz-Josef Fabry opines the two designations in the DSS refer to actual priestly groups, who—although related—were in some state of rivalry with each other. Fabry reiterates the oft-noted aspect that in the DSS the Sons of Aaron are associated more with cultic functions, while the Sons of Zadok with official-administrative functions. (27) For his part, Albert I. Baumgarten thinks that “[t]he Zadokite priests, whether real or invented to suit the need,” had only an “incomplete” success in taking control over the Dead Sea ‘sect’. (28)

c) *The Sons of Zadok in the DSS: Ideological and Eschatological Interpretations*

Already Solomon Schechter characterized the “spiritual pedigree” of the “sect” deriving from Zadokites and pointed out the latter’s eschatological function in CD. (29) Almost sixty years later, Jacob Liver made a similar statement and remarked that the “Zadokite priests are mentioned in conjunction with their position of authority within the sect and not in relation to cultic matters”. (30) According to Devorah Dimant, the eschatological function for the Sons of Zadok also exists in 4QFlor. (31) Otto Mulder has defended the same role in Sir 51:12a–o. (32) In fact, more scholars have stressed the designation

(26) See further Hempel, *Qumran Rule Texts*, 222–27.

(27) “Priests at Qumran,” in *The Dead Sea Scrolls: Texts and Context* (ed. Charlotte Hempel; STDJ 90; Leiden: Brill, 2010), 259–62 (with earlier literature). Fabry observes that out of 30 references, the Sons of Aaron are called “the priests” nine times, whereas the Sons of Zadok are described so seven times. Against Vermes, and like Hempel, Fabry thinks the two designations are not used synonymously and that the relation between the “two priestly traditions” is rather complex.

(28) “The Zadokite Priests at Qumran: A Reconsideration,” *DSD* 4/2 (1997): esp. 152–53.

(29) *Documents of Jewish Sectaries, I*, xxi (53). See further Ben Zion Wacholder, *The New Damascus Document: The Midrash on the Eschatological Torah of the Dead Sea Scrolls: Reconstruction, Translation and Commentary* (STDJ 56; Leiden: Brill, 2007), 177–84.

(30) “The ‘Sons of Zadok,’” 28. “The ‘Sons of Zadok, the Priests’ in the Dead Sea Sect,” *RevQ* 6/1 (1967): 10, 28–29. Both Liver and Schechter believed the Sons of Zadok could trace their genealogy to pre-exilic times.

(31) *History, Ideology and Bible Interpretation*, 287.

(32) “Three Psalms or Two Prayers in Sirach 51? The End of Ben Sira’s Book of Wisdom,” in *Prayer from Tobit to Qumran. Inaugural Conference of the ISDCL at Salzburg, Austria, 5–9 July 2003* (eds. Renate Egger-Wenzel and Jeremy Corley; DCL 1; Berlin: de Gruyter, 2004), 182–87.

“Sons of Zadok” should not be understood in a strictly genealogical way, and this applies to all DSS. Thus some scholars interpret it in a metaphorical (33) or ideological (34) sense; still others regard this appellation as an honorific title, (35) or as a “moral quality”. (36) That “Sons of Zadok” is used in an ideological sense not only in CD but also in 4QD^a 5 I 1–19 seems to be suggested by the following col. 5 II 1–16 (//4QD^b 5 III 1–8; 4QpapD^h 2; 4 I). The latter defines those “priests” disqualified from reading the Torah, approaching the holy service, or partaking of the sacred food. (37) This brief list of regulations four times refers to the Sons of Aaron only, while not once mentioning the Sons of Zadok. Were they not also “priests”? This aspect is even more striking as, shortly before, 4QD^a 5 I 16 speaks about the Sons of Zadok.

According to Robert A. Kugler, “the oft-stated view of the community being essentially a ‘priestly group’ originating from a withdrawal of Zadokite priests from the temple over Hasmonean seizure of the high priest’s office is ... undermined by the evidence”. Like Vermes, Kugler thinks the Sons of Zadok “were introduced into the ‘community texts’ only late in the recensional development”. However, in his view, the “priests emerged as important figures [both in Rule texts and in D] ... only in a literary world”. Hence, Kugler wonders whether “there were corresponding social realities to those expressed in the texts”. (38)

Joseph L. Angel, who focused on the otherworldly and eschatological dimensions of the priesthood in the DSS, argued that, besides CD, such dimensions in relation to the Sons of Zadok are to be found in 1QS, 1QSa, and especially 1QSB. With regard to 1QSa, its opening sets

(33) So, for example, Maxine L. Grossman, *Reading for History in the Damascus Document: A Methodological Method* (STDJ 45; Leiden: Brill, 2002), 187–88.

(34) So, for example, Nathan MacDonald, *Priestly Rule: Polemic and Biblical Interpretation in Ezekiel 44* (BZAW 476; Berlin: de Gruyter, 2015), 145.

(35) So, for example, John J. Collins, *Beyond the Qumran Community: The Sectarian Movement of the Dead Sea Scrolls* (Grand Rapids, MI/Cambridge, UK: W.B. Eerdmans, 2010), 97–98, 121; followed by Gillihan, *Civic Ideology*, 310.

(36) So, for example, Étienne Nodet, “Asidaioi and Essenes,” in Flores Florentino: *Dead Sea Scrolls and Other Early Jewish Studies in Honour of Florentino García Martínez* (eds. Anthony Hilhorst, Émile Puech, and Eibert J.C. Tigchelaar; JSJSup 122; Leiden: Brill, 2007), 67.

(37) For a comment, see earlier, Charlotte Hempel, *The Laws of the Damascus Document: Sources, Tradition, and Redaction* (STDJ 29; Leiden: Brill, 1998), 39–42.

(38) “Priesthood at Qumran,” in *The Dead Sea Scrolls After Fifty Years: A Comprehensive Assessment*, Vol. II (eds. Peter W. Flint and James C. VanderKam; with the assistance of Andrea E. Alvarez; Leiden: Brill, 1999), 113–14. Similarly, Philip R. Davies repeatedly claimed that it is unlikely the “sect” became engaged in national politics; see “What History Can We Get from the Scrolls, and How?” in *The Dead Sea Scrolls: Texts and Context* (ed. Charlotte Hempel; STDJ 90; Leiden: Brill, 2010), 31–46.

the eschatological scene: “This is the rule for all the congregation of Israel in the last days” (col. I 1). Angel notes how the ideological and eschatological role of the Sons of Zadok supersedes the genealogical claim. (39) However, Angel also persuasively concludes that in IQSa “the eschatological priesthood was modeled closely on the present realities of the community”. (40)

Are the Sons of Zadok used in historical, ideological and eschatological sense, or some combination of both? How to reconcile these interpretations with the literary studies? Therefore, a closer look at the Sons of Zadok both in Qumranic and extra Qumranic literature is necessary.

3. The Sons of Zadok, the Sons of Aaron, and the Second Temple Priesthood

Many scholars have assumed for decades that the high priests of Jerusalem were all Zadokites before the Hasmonean takeover. The study of Deborah Rooke is the most detailed in defending this view. Her main conclusion is that the Sons of Zadok was the common designation both for high priests and even priests, at least since the late monarchy. The last representative in the line was the Oniad high priest, Jason. (41) According to Cana Werman and other scholars, “all the priests of the Second Temple were members of the group ‘the Sons of Zadok’”. (42)

By contrast, Alice Hunt concluded “there was never a priestly dynasty until the Oniads [who] were ousted by ... the Hasmoneans” and that even this dynasty never called itself Sons of Zadok. A group which called itself thus existed at the time of Alexander Jannaeus (103–76 BCE) and maybe before, but they merely joined the *yahad*; they did not found it. (43)

(39) *Otherworldly and Eschatological Priesthood in the Dead Sea Scrolls* (STDJ 86; Leiden: Brill, 2010), 107–23, 166.

(40) *Otherworldly and Eschatological Priesthood*, 203.

(41) *Zadok's Heirs: The Role and Development of the High Priesthood in Ancient Israel* (OThM; Oxford: University Press, 2000), 16, 275, 281, 311; recently also Piotrkowski, *Priests in Exile*, 9, 384. For an overview of earlier scholarship, see Alice Hunt, *Missing Priests: The Zadokites in Tradition and History* (LHB/OTS 452; New York, NY: T&T Clark International, 2006), 1–49.

(42) “The Sons of Zadok,” in *The Dead Sea Scrolls: Fifty Years after Their Discovery 1947–1997. Proceedings of the Jerusalem Congress, July 20–25, 1997* (eds. Lawrence H. Schiffman, Emanuel Tov, and James C. VanderKam; Jerusalem: Israel Exploration Society & The Shrine of the Book, 2000), 629.

(43) For a survey of the non-biblical texts including the DSS, see Hunt, *Missing Priests*, 144–64, 190. One notes that in the Elephantine Jewish papyri the ministers of

Indeed, it appears that neither the pre-Hasmonean high priests, nor priests in general, regarded themselves as “Sons of Zadok” in a strictly genealogical sense. (44) First of all, this is evident from Ezek 40–48 itself, where the Sons of Zadok are introduced alternately as “Sons of Levi” (40:46) or as “Levitical priests” (43:19; 44:15). Second, Ezekiel himself is never referred to as belonging to the Sons of Zadok, or to the Sons of Aaron even. Third, the Priestly Torah became the authoritative Scripture for the priests of both the Jerusalem and Gerizim temples. Ezra–Nehemiah and 1–2 Chronicles also speak of priests as Sons of Aaron. First Chronicles 24:1–19 in particular, has Zadok descend from Aaron through Eleazar. As scholars have plausibly suggested, this list is the result of some compromise reached by that time between rival priestly groups. (45) However, even here the priests are listed as Sons of Aaron in the line of Eleazar and Ithamar (24:1; cf. 24:19). This list of priestly courses turns up in 4QMishmarot (4Q320–330), dated paleographically to the 2nd and 1st c. BCE. (46)

In his eulogy of the Fathers, Ben Sira does not mention Zadok but instead praises Aaron (45:6–22) and his grandson, Phinehas (45:23–25). Sirach 45:25 also specifies that Aaron’s heritage is for his descendants alone “from son to son”. Furthermore, in Sir 50 Simon (II) is depicted serving as high priest in the temple being surrounded by Sons of Aaron (50:12–16). The same holds true for Tob 1:7.

With the exception of 4QD^a 5 I 16, all other 4QD as well as 4QS texts mention the Sons of Aaron only. Important DSS such as the Temple Scroll (11QT^{a-b}), 4QMMT or the War Scroll (e.g. 1QM; 4Q493), which deal with the temple and the priesthood, also refer to priests only as Sons of Aaron. 4QMMT, which many scholars consider a sectarian composition, refers several times to the priests of the Jerusalem temple only as Sons of Aaron (MMT B 17, 79, 82). There are more DSS which use this designation when referring to priests in general.

A 2nd–1st c. BCE Paleo-Hebrew inscription translates: “I, Abba, son of the priest El’az(ar), son of Aharon” (CIII/P No. 55). Scholars

the temple of YHWH are called simply “priests” (e.g. TADAE I: No. 30). Likewise, there is no evidence that the priests in Leontopolis claimed a Zadokite descent.

(44) On ‘non-Zadokite’ high priests, see Maria Brutti, *The Development of the High Priesthood during the Pre-Hasmonean Period: History, Ideology, Theology* (JSJSup 108; Leiden: Brill, 2006), 110–15.

(45) See, for instance, Gary N. Knoppers, *1 Chronicles 10–29: A New Translation with Introduction and Commentary* (AB 12A; New York, NY: Doubleday, 2004), 841–42.

(46) Some of the list’s names appear on papyri and inscriptions: e.g. Eliashib (1 Chr 24:12) in PapMur 20 I 2; Yakim (1 Chr 24:12; CIII/P No. 183); Hezir (24:15; No. 137). A list of 24 priestly courses is known from an inscription from Caesarea (CIII/P No. 1145, 4th–5th c. CE).

debate whether the inscription is of Judean or Samaritan provenance, but more important here is the significance of the two names. Either this priest's father and grandfather carried such names, or "son of Aaron" is used here to claim priestly descent. "Aaron" as a personal name in the Second Temple period Palestine is otherwise unattested. (47) It appears only in several (mostly) post-biblical sources both from Palestine and the Diaspora. Even there it is ambiguous whether it reflects a personal name or evokes the biblical priestly figure. (48) By contrast, in these sources one does not encounter such formulas as 'X son of Zadok, the priest'.

There is ample evidence that the rules of the Priestly Torah prevailed during the Second Temple period: the high priests were believed to occupy the office of Aaron (see also 1 Macc 7:14; Josephus, *Ant.* 20.225–226), whereas the regular priests were usually referred to as Sons of Aaron. Since the label "Sons of Zadok" also appears in Ezek 40–48, some scholars have interpreted this as a Qumranic insertion.

4. The Sons of Zadok in Ezekiel 40–48: 'Qumranic' Insertions?

There are four references to the Sons of Zadok in MT: Ezek 40:46; 43:19; 44:15; 48:11. Several redaction-critical studies have concluded that even though part of the visionary language of Ezek 40–48 may date to the time of the prophet Ezekiel, the Sons of Zadok passages are additions made in one or more stages. Most scholars have dated such scribal activity between the end of the 6th and the 4th c. BCE. (49)

(47) Tal Ilan, *Lexicon of the Jewish Names in Late Antiquity, Part I: Palestine 330 BCE–200 CE* (TSAJ 91; Tübingen: Mohr Siebeck, 2002), 5–6.

(48) See *CIJ* Nos. 497, 1420; *IOJ* 3 No. Syr102; *CII/P* Nos. 441, 881; *CPR* 13 No. 12.6. For the latter 3rd c. BCE inscription from Egypt, see Hermann Harrauer (ed.), *Griechische Texte, IX: Neue Papyri zum Steuerwesen im 3. Jh. v. Chr.* (*CPR* 13; Wien: Österreichische Nationalbibliothek—Hollinek, 1987), 231, 234. The Samaritan sources mention only Pinhas: see Yitzhak Magen, *A Temple City*, Vol. 2 of *Mount Gerizim Excavations*. Translated from Hebrew by Edward Levin and Carl Ebert (Judea & Samaria Publications 8; Jerusalem: Israel Antiquities Authority, 2008), Nos. 24, 25, 384, 389. Pinhas evokes Aaronite lineage. No. 384 may even refer to a Pinhas as "high [priest]" (הַכֹּהֵן הַגָּדוֹל). One should neither expect the Samaritan sources to mention the Sons of Zadok, as Ezekiel 40–48 is a Jerusalem-centered vision.

(49) For the tendency to date the whole Book of Ezekiel to the 6th c., see Moshe Greenberg, *Ezekiel 1–20: A New Translation with Introduction and Commentary* (AB 22; Garden City, NY: Doubleday & Co., 1983), 11–17. By contrast, Walther Zimmerli, *Ezekiel 2: A Commentary on the Book of the Prophet Ezekiel, Chapters 25–48*. Translated by James D. Martin (Hermeneia. Philadelphia: Fortress Press, 1983), 553, assigns the passages containing the Sons of Zadok to the early post-exilic period; similarly, Steven Sh. Tuell, *Ezekiel* (NIBC; Peabody, MA: Hendrickson Publishers, 2009), 281, thinks the Zadokite passages date "probably during the reign of Darius I" (522–486 BCE);

Other scholars concluded that the Ezekiel textual tradition continued to develop down into the Hellenistic or even the Hasmonean period. (50) According to Alice Hunt, “[s]omeone, at an undetermined time, but prior to the ‘Zadokite’ insertion in *Serek ha-Yahad*, inserted material in Ezek 40–48 about the superiority of the ‘sons of Zadok’”. (51) This late scribal insertion hypothesis was also defended by Nathan MacDonald. (52) Thus, the hypothesis proposes that ‘Zadokite’ scribes are responsible for a proto-MT edition of Ezekiel.

While I partly agree with Hunt, namely that the Sons of Zadok were added later in the textual development of Ezekiel, I believe the references to the Sons of Zadok in Ezek 40–48 substantially predate those in the DSS. They should be dated either shortly before or about the same time as the Books of Chronicles. First, Hunt’s theory implies that in the 2nd c. BCE a group of scribes could control all extant Hebrew manuscripts of Ezekiel in and (perhaps) outside Judea. Such large-scale editorial control is highly unlikely, not least because the Sons of Zadok—or at least part of them—were very probably at odds with temple officials and the Hasmonean dynasty, as Hunt herself admits. (53)

Second, one must reckon with the great influence which the Book of Ezekiel exerted over many DSS, already before the Sons of Zadok were imported. Not only the so-called Old Greek (OG) version of Ezekiel—which itself likely predates the Zadokite insertions in the DSS—but also its Hebrew *Vorlage* must have already contained the Sons of Zadok references since they are found in the MT as well.

Michael D. Konkel, *Architektur des Heiligen: Studien zur zweiten Tempelvision. Ez 40–48* (BBB 129; Berlin: Philo, 2001), 285–86, 346–48, ascribes them to his “zweite Fortschreibung” layer which he dates to the second half of the 5th c.; Thilo A. Rudnig, *Heilig und Profan: Redaktionskritische Studien Ez 40–48* (BZAW 287; Berlin: de Gruyter, 2000), 364, places the Zadokite passages in the 4th c.; Janina M. Hiebel, *Ezekiel’s Vision Accounts as Interrelated Narratives: A Redaction-Critical and Theological Study* (BZAW 475; Berlin – Boston, MA: de Gruyter, 2015), 179, also sees these passages as late additions.

(50) See Ashley S. Crane, *Israel’s Restoration: A Textual-Comparative Exploration of Ezekiel 36–39* (VTSup 122; Leiden: Brill, 2008), 255–62; Daniel M. O’Hare, “*Have You Seen, Son of Man?*”: A Study in the Translation and Vorlage of LXX Ezekiel 40–48 (SBL.SCS 57; Atlanta, GA: SBL Press, 2010), 78, 137; William A. Tooman, *Gog of Magog: Reuse of Scripture and Compositional Technique in Ezekiel 38–39* (FAT/2 52; Tübingen: Mohr Siebeck, 2011), 270–74; Karl-Friedrich Pohlmann, “Ezekiel: New Directions and Current Debates,” in *Ezekiel: Current Research and Future Directions* (eds. William A. Tooman and Penelope Barter; FAT 112; Tübingen: Mohr Siebeck, 2017), 13.

(51) *Missing Priests*, 190.

(52) MacDonald, *Priestly Rule*, 145, 149–50.

(53) *Missing Priests*, 178, 190, against Liver, “The ‘Sons of Zadok,’” 3–30, who rejected anti-Hasmonean implications in the appellation Sons of Zadok.

Third, the two versions of Ezekiel do not display the same *Tendenz*. This led some scholars to point to Zadokite priestly scribes as being alternately responsible for the proto-MT or the OG. (54) It should be emphasized that in the LXX, the Sons of Zadok occur five times as compared with the four in the MT: see LXXEzek 42:13. According to Daniel M. O'Hare, at least some "supplements" or "pluses" in the LXX promote the social position of the "Zadokites" more than the MT does. (55) O'Hare even surmises "the possibility that the *Vorlage* of LXXEzek 40–48 was transmitted and studied in Zadokite circles". (56) Because these texts contain many other significant differences, the possible relation of the Zadokite DSS to LXXEzekiel deserves a separate study. For the moment, I am only arguing that the Sons of Zadok were an integral part of the Ezekiel textual tradition before the Zadokite DSS were produced.

Next, we shall briefly examine the main ideological themes surrounding the Sons of Zadok in Ezek 40–48. The premise is that the Sons of Zadok in the DSS should not be interpreted in isolation from these themes and their source-context.

5. The Sons of Zadok and Their Ideological Claims in Ezekiel 40–48 (MT&LXX)

Ezekiel 40–48 reports a vision of the establishment of post-exilic institutions. Among others, this utopian plan focuses on the new city, its new temple, priesthood, and model of rule. This vision makes three ideological claims which are relevant here

The first ideological claim regards the place which should house the new temple of YHWH. Although Jerusalem is not explicitly mentioned

(54) For the view that LXXEzekiel is anti-Hasmonean, see Johan Lust, "Messianism in LXX-Ezekiel: Towards a Synthesis," in *The Septuagint and Messianism* (ed. Michael A. Knibb; BETL 195; Leuven: Peeters, 2006), 417–30. For the view that the LXXEzek is pro-Hasmonean and the proto-MT is pro-Zadokite, see Arie van der Kooij, "The Septuagint of Ezekiel and Hasmonean Leadership," in *Interpreting Translation: Studies in the LXX and Ezekiel in Honour of Johan Lust* (eds. Florentino García Martínez and Marc Vervenne; BETL 192; Leuven: Peeters, 2005), 437–46; Michael D. Konkel, "Das Ezechielbuch zwischen Hasmonäern und Zadokiden," in *Juda und Jerusalem in der Seleukidenzeit: Herrschaft—Widerstand—Identität. Festschrift für Heinz-Josef Fabry* (eds. Ulrich Dahmen and Johannes Schnocks; BBB 159; Göttingen: V&R & Bonn University Press, 2010), 65–75.

(55) "Have You Seen, Son of Man?", 75, 122–23, 191–92.

(56) "Have You Seen, Son of Man?", 138. This suggestion seems to have been tacitly accepted by Johan Lust in his review of O'Hare's book in *ETL* 87/1 (2011): 252–54. Corrado Martone, "From Universal to Sectarian: The Zadokites, Qumran, the Temple, and Their Libraries," *Hen* 40/1 (2018): 25–27, even suggests a "Zadokite" library may have existed once at the temple.

after Ezek 36:38 (MT), it is plausible to think that, according to Ezek 40–48, it is the same city. There is some reason to believe that this claim should be read in the light of such texts as Deut 12:5 and parallels, which talk about the disputed “place” of dwelling for God’s Name. (57) The new temple is sanctioned by God through Ezekiel’s visions and is sanctified by the return of YHWH’s Glory as described in Ezek 43:1–5.

The second ideological claim deals with those who should serve in this imagined temple. According to Ezekiel’s visions, these are the Levitical priests, the Sons of Zadok, and not the Sons of Aaron, who are the only legitimate priests in the Priestly Torah and elsewhere (e.g. 2 Chr 26:18; Sir 50:13, 16). The exclusive right of the Sons of Zadok is, again, sanctioned by God. It is worth noting that, although Ezekiel himself is introduced as a priest, the son of Buzi, in Ezek 1:3, he is not associated with the Sons of Zadok. (58) In a forthcoming article, I argue that the insertion of the Sons of Zadok into Ezekiel should be understood in an ideological—not a genealogical—sense. This designation was meant to reclaim the expectation of a Davidic leader in order to counter the ideological claims of the Sons of Aaron.

The third ideological claim concerns the presence of a non-priestly figure who is mentioned some eighteen times in Ezek 40–48; considerably more than in chs. 1–39 (MT). This disputed “Prince” (נשיא) should also have his place and role in the new temple among the people of Israel (cf. Ezek 45:16). That a Davidic figure is envisioned in Ezek 40–48 is suggested by divine promises in previous passages which refer to him as either נשיא (34:23–24; 37:25) or מלך (37:22–24). (59) Additionally, scholars have noted that LXX⁹⁶⁷ in particular has a much more pronounced ideology of a Davidic Messiah than the MT (e.g., LXXEzek 17:23; 21:30–32; 43:3). (60) Further, Ezek 37:22, 24 (MT) refers to him as מלך, while the LXX has ἄρχων. Johan Lust thinks the *Vorlage* of LXXEzek contained נשיא throughout, which is the only term used for the leader in Ezek 40–48. (61)

(57) See also Deut 12:11, 14, 18, 21, 26; 14:23, 24, 25; 15:20; 17:8, 10; 18:6; 26:2; 31:11.

(58) Likewise, the priest and the prophet Jeremiah is associated neither with the Sons of Zadok nor with the Sons of Aaron (cf. Jer 1:1).

(59) See Daniel I. Block, *Beyond the River Chebar: Studies in Kingship and Eschatology in the Book of Ezekiel* (Cambridge, UK: James Clark & Co., 2014), 74–94; Franz Sedlmeier, “The Figure of David and His Importance in Ezekiel 34–37,” in *Ezekiel: Current Research and Future Directions* (eds. William A. Tooman and Penelope Barter; FAT 112; Tübingen: Mohr Siebeck, 2017), 92–106.

(60) See, for example, Ingrid E. Lilly, *Two Books of Ezekiel: Papyrus 967 and the Masoretic Text as Variant Literary Editions* (VTSup 150; Leiden: Brill, 2012), 302.

(61) “Messianism in LXX-Ezekiel,” 422–25. According to Lust, 426, this difference may be due to “another translator” in Ezek 40–48, or due to the fact that “the translator

These three ideological claims are especially important, as they call for a leader who should rule over united Israel, and for one temple only. (62) This imagined figure appears to have considerably fewer powers than pre-exilic Davidic kings. His powers within the temple, and perhaps even outside it, are limited by the Sons of Zadok even more in the LXX. By contrast, while there is no clearly identifiable high priestly figure in Ezek 40–48, the role of the Sons of Zadok is prominent throughout, and more so in the LXX. For example, LXXEzek 44:24 in particular, assigns the Sons of Zadok the right to serve as judges in capital cases.

It follows that, in Ezek 40–48, the Sons of Zadok—referring to priests—are associated with a future, probably Davidic figure, and with a future temple. Most importantly, it portends a separation of the power of the national leader from temple issues. This civic ideology should be understood not just as an utopian restoration of pre-exilic institutions, (63) but also in the light of the gradual emerging prerogatives of the post-exilic chief priest. (64) Can we identify similar ideological elements in the DSS? (65)

6. The Sons of Zadok, the Hasmonean Rule, and the Ideology of Two Messiahs

The Damascus Document is rich in messianic expectations. It refers to David three times, but not in eschatological contexts. However, it refers four or perhaps five times to expectation of a “Messiah (sg.) of Aaron and Israel”: e.g., CD 1:7 [?]; 12:23–13:1; 14:19//4QD^a 10 I 12//4QD^d 11 I 2; CD 19:10–11; 20:1 (“Messiah from Aaron and from Israel”). While some scholars reasoned it refers to a single Messiah, (66) most others defended the theory of two Messiahs: a priestly and a royal one. (67) Hartmut Stegemann assigned the expectation of two Messiahs

wished to distinguish between the messianic saviour in Ez 34 and 37, and the leader ... described in 40–48.” Earlier, Lust, “Ezéchiél dans la Septante,” in *Les recueils prophétiques de la Bible: Origines, milieu, et contexte proche-oriental* (Le monde de la Bible 64; Genève: Labor et Fides, 2012), 348, was “en faveur d’un traducteur unique”.

(62) The term “Israel” is used over thirty times in Ezek 40–48 alone.

(63) So, for example, Rooke, *Zadok’s Heirs*, 119.

(64) So, for example, MacDonald, *Priestly Rule*, 53. On the possibility that Ezek 45:19 implies the function of a chief priest, see Iain M. Duguid, *Ezekiel and the Leaders of Israel* (VTSup 56; Leiden: Brill, 1994), 63–64.

(65) There are other biblical texts which speak, in one way or another, about these aspects. However, none of them is so extensive and has influenced so much the DSS as Ezek 40–48, and not only because it mentions the Sons of Zadok.

(66) So, for example, George J. Brooke, “The Messiah of Aaron in the Damascus Document,” *RevQ* 15 (1991): 215–30.

(67) So, among others, John J. Collins, *The Scepter and the Star: Messianism in Light of the Dead Sea Scrolls*. Second edition (Grand Rapids, MI/Cambridge, UK: W.B. Eerdmans, 2010), 79–108, esp. 84–91.

to his “third stage of development” of messianic ideology, which, as he claimed, was elaborated from about 100 BCE onwards. Stegemann posited that before this period only a royal Messiah was expected. (68) John J. Collins convincingly reiterated the view that expectation of a Messiah of Israel and Aaron “was in itself a political statement, since it implicitly rejected the combination of royal and priestly offices by the Hasmoneans”. Collins further clarifies that “[t]he insistence on dual messianic offices of priest and king implies a critique of the combination of these offices by the Hasmoneans”. (69)

The same anti-Hasmonean ideology appears to be acclaimed in 1QS. It speaks of expectation of the “Messiahs” (pl.) of Aaron and Israel, which implies the separation of the high priestly office from the civil one (1QS 9:11). It is worth noting that already Jean Starcky dated the beginning of the bi-Messianism ideology in 1QS to the time of Alexander Jannaeus (“Phase II”). (70) Next to it there is the expectation of a Prophet too. Several scholars have characterized this line as part of an editorial addition. (71) First, it is paralleled in 4QTest, which may have been its source. (72) Second, the text of 1QS 8:15b–9:11 as a whole is lacking in the arguably older exemplar 4QS^c, which instead preserves parallels to 1QS 8:11–15a and 9:12–20. (73) Hartmut Stegemann argued that the ideology of 1QS 9:11/4QTest is a later development than 1QSa and 1QSB in their present form and should be dated from 100 BCE onward. (74) John J. Collins also theorizes that 1QS 9:11 “was added at a later stage in the development of 1QS”. (75) If, as I argue here, the Sons of Zadok passages (i.e. 5:2, 9; 9:14) are also later, then it could be that they were introduced in 1QS together with, or shortly after, the “Messiahs of Aaron and Israel” passage in col. 9:11.

(68) See further “Some Remarks,” 501–5.

(69) *The Scepter and the Star*, 108–9.

(70) “Les quatre étapes du messianisme à Qumran,” *RevB* 70/4 (1963): 487. However, for Starcky in D the “Messiah of Israel and Aaron” reportedly dates to the Pompeian period (“Phase III”) because here he sees one Messiah only. Starcky’s reconstruction is tightly connected with the archaeological phases of Qumran proposed by De Vaux.

(71) So already Starcky, “Quatre étapes du messianisme,” 487.

(72) The textual relation between 1QS and 4QTest requires further analysis which is beyond our scope here. 4QTestimonia gathers scriptural evidence that lay basis apparently for three distinct eschatological figures: King, Priest, and Prophet. Some scholars think it is an indirect criticism against Hyrcanus I, who reportedly claimed all three privileges (cf. *Ant.* 13.299–300). See further Kenneth Atkinson, “John Hyrcanus as a Prophetic Messiah in 4QTestimonia (5QTest),” *QC* 24/1 (2016): 9–27. Yet this interpretation is not easy to reconcile with texts like 1 Macc 4:46 and 14:41, compiled probably towards the end of the 2nd c. BCE.

(73) See Metso, *Textual Development*, 71–73.

(74) “Some Remarks,” 479, 504–5,

(75) *The Scepter and the Star*, 91.

Like 1QS and 1QSa (below), 1QSa does not mention David. Yet 1QSa refers twice to the “Messiah of Israel” (2:14, 20), meaning royal or lay Messiah. In light of the newly reconstructed 4QSE (4Q249a; see below), Émile Puech has reconfirmed that 1QSa 2:11–12 refers to a priestly Messiah too. Puech contends that “the Priest” in 2:19 is the same figure. (76) While the royal Messiah is in the company of his men and elders of Israel, the priestly Messiah is in the company of priests, “[from the Sons of] Aaron”. This points to the authority of the (high) priest over priests, which thus implies their separation from the royal power. The priestly Messiah is the first to enter the messianic banquet scene and seems also to preside over it. (77) According to Johannes Zimmermann, this Messiah may possibly be identified with the anointed of Aaron. (78) Hence, 1QSa too seems to entail a separation of offices. These elements also suggest the Sons of Zadok passage in 1QSa 3:22–24 was added probably shortly thereafter.

Both 1QSa 2:5 and 5:21 speak of “kingship/kingdom forever”. 1QSa 5:24, 27 also mention a “scepter” and the same column contains other royal symbolism. Indeed, the whole of col. 5 appears to be a benediction of the “Prince [נשיא] of the Congregation”, very probably a royal Messiah. By contrast, the previous column appears to be blessings of the high priest (or priestly Messiah?) since 1QSa 4:28 mentions the “diadem [נוֹר] of the Holy of Holies”. Hartmut Stegemann, drawing on the reconstruction of 1QSa by Józef T. Milik in DJD 1, has isolated cols. 1:19–3:19 as the (second) blessing of the high priest, cols. 3:20–5:17 as a (third) blessing of the Sons of Zadok, and col. 5:18–27 as the (fourth and concluding) blessing of the Prince (נשיא) of the Congregation—whom he has identified as a royal Messiah. (79) By contrast, Martin G. Abegg Jr. has isolated only cols. 3:22–4:19 as the (fourth) blessing on Zadokite priests, while cols. 4:20–5:19 as the (fifth) blessing of the high priest, and cols. 5:20–6:20 (?) as the (sixth) blessing of the Prince. (80) The structure proposed by Abegg is more persuasive, but

(76) “La préséance du Messie prêtre en 1Qsa II 11–22,” *RevQ* 30/1 (2018): 85–89.

(77) See Puech, “Préséance sacerdotale et Messie-Roi dans la Règle de la Congrégation (1Qsa ii 11–22),” *RevQ* 16/3 (1994): 351–65. For a slightly different approach, see earlier Stegemann, “Some Remarks,” 491–95, who argued, among others, that 1QSa 2:1–22 is a reworked parallel of 1QS 6:2–6 and that these passages contain both the present as well as eschatological regulations.

(78) *Messianische Texte aus Qumran: Königliche, priesterliche und prophetische Messiasvorstellungen in den Schriftfunden von Qumran* (WUNT 104; Tübingen: Mohr Siebeck, 1998), 35; see also the discussion on pp. 23–35, also in reference to 1QS 9:11.

(79) “Some Remarks,” 498–99.

(80) “1QSa and the Elusive High Priest,” 8.

both Stegemann and Abegg are right to interpret the figures in 1QSb in correlation with 1QS, and especially with 1QSa, where a royal Messiah is expected. (81) Whether we read 1QSb together with—or isolated from—1QS and 1QSa, a separation of the high priestly office from the royal office is clearly envisaged in 1QSb 4, just as in 1QSa and 1QS. Because these texts were copied by the same scribe and may have been part of the same scroll, one can argue that they were meant to be read together.

Curiously, James H. Charlesworth isolates 1QSb 4:22–28 as a separate blessing of the “Zadokite” high priest and claims it to be a polemic against the Hasmoneans because, he argues, it implies the Sons of Zadok: (1) are to bless the people; (2) the Prince (נשיא) should not control the priests; (3) the high priest is to come from the Sons of Zadok (4:28). (82) This apparent polemical tone persists if, with Abegg, one interprets all of 1QSb 4:20–5:19 as the blessing of the high priest. The possibility that this high priest is related to the Sons of Zadok still holds, as the surviving text of 1QSb mentions neither Aaron nor the Sons of Aaron. If this could be proven, such a notion would be unique, seeing that in the DSS, as elsewhere, the high priest is usually associated with the office of Aaron and not Zadok. (83)

For its part, 4QFlor speaks about the rise of the “shoot” (צמח) of David, whose name is stressed four times in this *peshar* (1–2 I 7–13): three times as part of the interpretation of biblical quotations and once in a quotation taken from Amos 9:11. 4QFlorilegium uses three other correlative nouns to stress Davidic kingship: “house”, “seed” and “son”. George J. Brooke, among others, understands the “Interpreter of the Law” (1–2 I 11; also 23 I) as alluding to a priestly Messiah. A reference to both royal and priestly Messiah may be found, in fact, in frg. 5 line 3, which appears to read “I]srael and Aaron”. Both figures are eschatological in this *peshar*. (84) Here too, the expectation of two Messiahs implies a separation of high priestly and royal offices. The Sons of Zadok are introduced shortly after this messianic passage (1–2 I 17) in a context in which the *yahad* understands itself as living “in the last days”.

(81) Stegemann, “Some Remarks,” 499–505; Martin G. Abegg, “1QSb and the Elusive High Priest,” in *Emanuel: Studies in Hebrew Bible, the Septuagint and the Dead Sea Scrolls in Honor of Emanuel Tov* (eds. Shaul M. Paul, Robert Kraft, and Lawrence H. Schiffman; VTSup 94; Leiden: Brill, 2003), 9–16.

(82) Charlesworth and Stuckenbruck, “Blessings (1QSb),” 120.

(83) For an evaluation of various interpretations of the blessing of the high priest in 1QSb, see Zimmermann, *Messianische Texte*, 277–85.

(84) *Exegesis at Qumran: 4QFlorilegium in Its Jewish Context* (JSOTSup 29; Sheffield: JSOT, 1985), 197–205. See also Zimmermann, *Messianische Texte*, 99–113; Dimant, *History, Ideology and Bible Interpretation*, 283–85.

The poorly preserved 4Qpap pIsa^c does not contain any mention of a Messiah. If, as some scholars have argued, its author/s commented on selected passages from Isaiah 8–31, then one cannot exclude the possibility that 4Qpap pIsa^c may originally have also interpreted such texts as Isa 11. (85) In fact, another exemplar, 4QpIsa^a (4Q161) 8–10 III 11–29, foresees the rise of a Davidic figure as part of the interpretation of Isa 11:1–5. (86) David will appear on a “throne of glory” with a “holy diadem” (נֹר הַקֶּדֶשׁ), dressed in “garments of variegated stuff”, with a [scepter?] “in his hand”, and “will rule over all the nations”.

Many scholars have recognized striking parallels with Sefer ha-Milḥamah (4Q285): 4QpIsa^a 8–10 III//4Q285 7, 4QpIsa^a 2–6 II//4Q285 4. (87) Both scrolls envisage a militant Messiah “in the last days”, a war with “Kittim”, and the establishment of Davidic rule over the Gentiles. This reading “in dialogue,” developed by George J. Brooke, led Alex P. Jassen to suggest that “4Q285 7 4–5 demonstrates that the Prince of the Congregation in 4Q161 A 5–6 3=H 2–6 II, 19 should be identified with the Branch of David in 4Q161 8–10 III, A 17=H 22.” (88) Both 4Q285 and 4QpIsa^a emphasize the leadership of the priests along with the Prince of the Congregation. The priests share some martial responsibilities on the battlefield. It is “one of the priests of renown” who gives the Messiah the garments before the start of the eschatological war (4QpIsa^a 8–10 III 29). These garments combine both royal and priestly elements. This may suggest that the prince should act in concert with—or even under—the authority of the priests. (89) Such a prince–priest/s relationship has been detected above in 1QSa, 1QSB, and 4QFlor.

The Sons of Zadok were imported into some DSS likely about the same time or shortly after the “Messiah/s of Aaron and Israel” ideology. Therefore, this appellation should be interpreted as a corollary

(85) 4Qpap pIsa^c frgs. 6–7 ends with a quotation of Isa 10:24, whereas frgs. 8–10 resume from Isa 14:8.

(86) The handwriting of 4QpIsa^a is usually dated to the Herodian period. For editions, see J. M. Allegro, “161. Commentary on Isaiah (A),” in DJD 5, 11–15; Maurya P. Horgan, “Isaiah Peshier 1 (4Q161 = 4QpIsa^a),” in DSSHAGT 6B, 83–97. Perhaps of the six *pesharim* on Isaiah, no clear textual overlap was preserved. There is also no clarity as to the number of exemplars. Thus, it has been noted that 4QIsa^a 2–6 II and 4QIsa^c 6–7 II contain contrasting interpretations of Isa 10:22–24.

(87) For a very detailed analysis, see Zimmermann, *Messianische Texte*, 59–96.

(88) “Re-Reading 4QPesher Isaiah A (4Q161) Forty Years After DJD V,” in *The Mermaid and the Partridge: Essays from the Copenhagen Conference on Revising Texts from Cave Four* (eds. George J. Brooke and Jesper Høgenhaven; STDJ 96; Leiden: Brill, 2011), 78. Jassen proposes a different arrangement of some 4QpIsa^a fragments as well as some new readings and sigla.

(89) See also Angel, *Otherworldly and Eschatological Priesthood*, 196–202; Jassen, “Re-Reading 4QPesher Isaiah A,” 83–90.

of the expectation of two Messiahs, which implies separation of the high priesthood from kingship. It is perhaps no coincidence that CD-A, 4QD^a, and 1QS are the only scrolls where the Messiah/s of Aaron and Israel on the one hand, and the Sons of Zadok on the other, are clearly attested. And 1QSa is the only scroll which mentions both the Messiah of Israel and the Sons of Zadok. In Stegemann's view, 1QSa and 1QS 5–11 (except 1QS 8:15b–9:11) predate D. (90) In line with this interpretation, Annette Steudel claimed that 1QS 5:1–7a in particular was rewritten in CD 3:12b–4:12b. Thus, she postulates that the Sons of Zadok in CD 3–4 were imported from 1QS 5:2. (91) This is a possibility, though it does not yet explain their occurrence elsewhere, either in these scrolls (e.g., 4QD^a 5 I 16; 1QS 5:9; 9:14) or in the others. Indeed, there is much parallel material between S and D, but a discussion of intertextuality, as well as of the theoretical common source used by both, would take us far beyond our present scope. (92)

We shall now briefly discuss concepts of a future temple in the same scrolls, as another element drawn from Ezek 40–48.

7. The Sons of Zadok and the Temple in Some DSS

The authors behind the Damascus Document still recognize the authority of the Jerusalem temple and allow their members to bring sacrifices there in fulfillment of the Law (CD 6:11–20; 9:13–15; 11:17–12:2; 16:13–14). However, they condemn those who are running the temple's affairs, depicting them as corruptors who contaminate the sanctuary (4:12–18; 6:13–20; 20:22–24).

A significant shift in the perception of the temple is noted in 1QS. Its authors depict the *yahad*, or rather its leadership, as a spiritual temple (8:4–10; 11:8–9) and judge a rightly-offered prayer as superseding temple sacrifices (9:3–6; see also 3:6–9; 5:1–7; 8:6–7, 10). These passages may suggest a complete rupture from the Jerusalem temple. (93)

(90) "Some Remarks," 479, 503–4.

(91) "The Damascus Document (D) as a Rewriting of the Community Rule (S)," 605–20, esp. 612.

(92) For a sketch of parallel passages, see Eibert J.C. Tigchelaar, "Annotated Lists of Overlaps and Parallels in the Non-biblical Texts from Qumran and Masada," in *The Texts from the Judaean Desert: Indices and an Introduction to the Discoveries in the Judaean Desert Series* (eds. Martin G. Abegg et al.; DJD 39; Oxford: Clarendon press, 2002), 319–20.

(93) George J. Brooke, "The Ten Temples in the Dead Sea Scrolls," in *Temple and Worship in Biblical Israel* (ed. John Day; LHB/OTS 422; London: T&T Clark International, 2005), 431, dates this changing perception of the temple "towards the end" of the 2nd c. BCE.

According to Devorah Dimant, 4QFlor 1–2 I 1–7 envisages three types of temples: “sanctuary of Israel”—probably referring to the existing temple in Jerusalem which the *pesharist* regards as polluted; an eschatological temple built by God (?) (see also 11QT^a 29:7–10); “a sanctuary of Men”—seemingly referring to those who practice the Law, i.e. the *yahad*. (94) The Sons of Zadok appear to be associated with a temple in “the last days” (1–2 I 15, 19) which contrasts with the (historical) temple of Israel.

Like some other Zadokite DSS, 4Qpap pIsa^c does not preserve clear evidence of temple-concepts, but 4QpIsa^d (4Q164) 1 1–8 appears to display a language which may allude to it. While interpreting Isa 54:11–12, it speaks—apparently in an eschatological context—of priests as the foundation of the *yahad* (=temple?), whereas the rest of the people are part of this temple (?). (95)

Because Sir 51:12a–o which mentions the Sons of Zadok, may also have originated from the same milieu, it is important to look at the way this text perceives both the Messiah and the temple.

8. The Sons of Zadok, the Messiah, and the Temple in Sir 51:12a–o

The dominant themes of the thanksgiving prayer in Sir 51:12a–o recall those of Ezek 40–48: the return of the exiles, the rebuilding of the city, the rebuilding of the temple, the rise of a Davidic ruler, and especially the election of the Sons of Zadok as priests. It praises God for building His sanctuary and for choosing Zion but associates them with the Sons of Zadok only. The hymn ends as follows: “Give thanks to Him who makes a horn to sprout for the house of David, for his mercy endures forever; Give thanks to Him who has chosen the Sons of Zadok to be priests” (Sir 51:12h–i).

Ben Sira 51:12a–o appears to be modelled on the canonical Psalm 136 (MT). It is called an ‘inset psalm’ because it appears between verses 12 and 13 of Sir 51 (MS B). Because its text is attested neither in other Hebrew manuscripts nor in the Greek or Syriac versions, some scholars doubt it was part of the original Hebrew Ben Sira. (96) Ben

(94) Dimant, *History, Ideology and Bible Interpretation*, 269–82. See also Angel, *Otherworldly and Eschatological Priesthood*, 87–97, 237–42; Cecilia Wassén, “End Time Temples in the Dead Sea Scrolls: Expectations and Conflict,” in *Apocalyptic Thinking in Early Judaism: Engaging with John Collins’ The Apocalyptic Imagination* (eds. Cecilia Wassén and Sidnie White Crawford; JSJSup 182; Leiden: Brill, 2018), 55–87.

(95) See further Maurya P. Horgan, *Pesharim: Qumran Interpretations of Biblical Books* (CBQMS 8; Washington, DC: The Catholic Biblical Association of America, 1979), 125–31.

(96) See Patrick W. Skehan, and Alexander A. Di Lella, *The Wisdom of Ben Sira* (AB 39; New York, NY: Doubleday, 1987), 568–71; Hunt, *Missing Priests*, 155–57.

Zion Wacholder suggests this text may equally belong after 68 CE as part of the “continuous scribal tradition” witnessed in Ben Sira (MS B) “from Qumran to the eleventh century”. (97) By contrast, both Otto Mulder and Francois Mies claim the contrary, arguing that the hymn was omitted by Ben Sira’s grandson in the translation process. (98) Alexander A. Di Lella noted some connection between certain themes of Sir 51:12a–o on the one hand, and CD and 1QS on the other. He suggested a Qumran origin for the psalm, but believes it is a pre-152 BCE Zadokite criticism at the ‘non-Zadokite’ Hasmoneans. (99)

Just as was the case with the CD (A&B), Sir 51:12a–o also may well have originated from the same Zadokite milieu. Ben Sira 1–50 is far from being a pro-Zadokite composition, and not only because it does not mention Zadok. This is especially evident in the praise of Aaron in 45:6–22, to whom it dedicates more space than to Moses. Sirach 45:25 specifies that Aaron’s heritage is for his descendants alone “from son to son,” i.e. perpetual. Curiously, Davidic covenant seems to extend only to Solomon. Further clarification comes from Sir 50, where Simon (II) is portrayed serving as high priest in the temple surrounded by the Sons of Aaron (50:12–16). (100) There is thus a contrasting ideology between Sir 45 and 50 on the one hand, and Sir 51:12a–o on the other, which suggests a different origin and very probably a later composition date for the latter.

The next question is whether there are any clues for identifying the period, at least approximately, when the Sons of Zadok were imported into the DSS.

(97) “Historiography of Qumran,” in *Qumran Between the Old and the New Testaments* (eds. Frederick H. Cryer and Thomas L. Thompson; JSOTSS 290/CIS 6; Sheffield: Academic Press, 1998), 363.

(98) Mulder, “Three Psalms,” 182–87; Mies, “Le Psaume de Ben Sira 51,12a–o Hébreu. Le Hymne aux Noms divins (Deuxième partie),” *RevBib* 116/4 (2009): 481–504.

(99) *The Hebrew Text of Sirach: A Text-Critical and Historical Study* (London: Mouton & Co., 1966), 101–5. For Qumranic origin of Sir 51:12a–o, see also Lutz Schrader, *Leiden und Gerechtigkeit: Studien zu Theologie und Textgeschichte des Sirachbuches* (BET 27; Frankfurt am Main: Peter Lang, 1994), 74–75.

(100) See Vasile Babota, *The Institution of the Hasmonean High Priesthood* (JSJSup 165; Leiden: Brill, 2014), 276. On Ben Sira as pro-Aaronide, see Saul M. Olyan, “Ben Sira’s Relationship to the Priesthood,” *HTR* 80/3 (1987): 261–86; Heinz-Josef Fabry, “Jesus Sirach und das Priestertum,” in *Auf den Spuren der schriftgelehrten Weisen. Festschrift für Johannes Marböck anlässlich seiner Emeritierung* (eds. Imtraud Fischer, Ursula Rapp, and Johannes Schiller; BZAW 331; Berlin and New York, NY: de Gruyter, 2003), 265–82; Benjamin G. Wright, *Praise Israel for Wisdom and Instruction: Essays on Ben Sira and Wisdom, the Letter of Aristeas and the Septuagint* (JSJSup 131; Leiden: Brill, 2008), 100–104.

9. Towards Dating the Zadokite Scribal Activity in the DSS

All or almost all Zadokite DSS are very probably based on earlier exemplars. (101) During the copying and revision process, some new material was likely added. The present aim is to identify the likely period when the Sons of Zadok became part of these documents. We shall proceed by putting in conversation the results of some redaction-critical studies with paleographic proposals. There is reason to suppose that all these scrolls were produced by the same small circle of Zadokite scribes.

Most redaction-critics have placed the ‘final’ composition of D by 100 BCE, or shortly later, (102) whereas its earliest textual layers may derive from as early as mid-2nd c. BCE. (103) Scholars who studied the D material in relation to S—whose textual growth is more complex—also supported this dating. (104) As far as the language of D is concerned, it has been observed that, while it is akin to that of Ben Sira, the Temple Scroll, and other known compositions from the 2nd c. BCE, it also preserves minor features found in Mishnaic Hebrew. (105)

Earlier paleographers had suggested that 4QD^a (4Q266) is the oldest D exemplar and dated it to the first half, (106) while others to

(101) On the identification of scribal hands, their *Vorlagen*, and possible autographs among the DSS, see the survey by Tov, *Scribal Practices*, 20–30. That the scribe of 1QS relied on *Vorlagen*, see earlier Tigchelaar, “In search of the Scribe,” 7–12. It has been observed that 1QS cols. 7–8 seems to betray traces of two different scribal hands, the second of whom may have done some corrections based on an earlier exemplar, unknown to us.

(102) For various views, see Hartmut Stegemann, “Towards Physical Reconstructions of the Qumran Damascus Document Scrolls,” in *The Damascus Document: A Centennial of Its Discovery. Proceedings of the Third International Symposium of the Orion Center, 4–8 February 1998* (eds. Joseph M. Baumgarten, Esther G. Chazon, and Avital Pinnich; STDJ 34; Leiden: Brill, 2000), 200 n 80; Charlotte Hempel, *The Damascus Texts* (CQS 1; Sheffield: Academic Press, 2000); Cecilia Wassen, *Women in the Damascus Document* (SBLAC 21; Atlanta, GA: SBL Press, 2005), 24. The concept of a final composition or edition is not satisfactory here as it implies there had been no ‘corrections’ done afterwards. Also, even though the D exemplars are more homogenous than those of S, yet we ought not assume they must have developed all in a linear way.

(103) See already Jerome Murphy-O’Connor, “A Literary Analysis of Damascus Document XIX,33–XX,34,” *RevB* 79/4 (1972): 544–64. See also Wacholder, *New Damascus Document*, 3–4; Hempel, *Damascus Texts*, 23, 44–53; Hultgren, *From the Damascus Covenant*, 540–42.

(104) The important question of the relation between D and 1QS is not within the specific scope of this study. See the very short survey by Stephen Hultgren, “Serekh ha-Yahad (S),” in *T&T Clark Companion to the Dead Sea Scrolls* (eds. George J. Brooke, Charlotte Hempel, et al.; London: T&T Clark, 2019), 344–46.

(105) See Steven E. Fassberg, “The Linguistic Study of the Damascus Document: A Historical Perspective,” in *The Damascus Document*, 53–68.

(106) DJD 18, 30: “first half or to the middle of the first century BCE”.

the second half of the 1st c. BCE. (107) By contrast, one radiocarbon dating showed it to be significantly later (1 σ : 5–80 CE; 2 σ : 45 BCE–120 CE). (108) 4QD^b (4Q267) 5 ii preserves partial parallel with 4QD^a 5 i 8–13 but one cannot infer whether its complete text mentioned the Sons of Zadok too (see 4QD^a 5 i 16). The proposed paleographic age for 4QD^b is the second half of the first c. BCE. (109) The radiocarbon dating, however, showed its material may stem from much earlier (1 σ : 172–98 BCE; 2 σ : 194–45 BCE). (110) There is thus a discrepancy between the two dating methods for both 4QD scrolls.

As for 1QS, 1QSa, and 1QSB, most redaction-critical studies have concluded that these achieved their form more or less as we know them by the early 1st c. BCE. To be sure, any attempt at dating their proto-versions should take into consideration the dating of the foundation of the *yahad*. However, the S textual tradition itself had started to develop before 100 BCE, almost certainly before any sectarian group decided to settle at Qumran. (111) Recently, Michael Brooks Johnson has contended that all three *Serekh* texts belong to the same composite work. For him, an “editor” merged them together by way of reframing the Maskil material and by introducing the Sons of Zadok. (112) If correct, this would support our view that the Sons of Zadok passages were among the latest insertions in the textual tradition of these scrolls.

Stephen J. Pfann published 23-minute papyrus fragments from Cave 4 which he tentatively assigned to eight or nine 4QSa exemplars. Two fragments which Pfann assigned to two different scrolls preserve

(107) “The Messiah of Aaron in the Damascus Document,” *RevQ* 15/2 (1991): 215–16, 230.

(108) For details and problems related to the radiocarbon tests conducted on several DSS, see Timothy A.J. Jull, Douglas J. Donahue, Magen Broshi, and Emanuel Tov, “Radiocarbon Dating of Scrolls and Linen Fragments from the Judean Desert,” *Radiocarbon* 37 (1995): 11–19; Gregory Doudna, “Dating the Scrolls on the Basis of Radiocarbon Analysis,” in *The Dead Sea Scrolls after Fifty Years* (eds. Peter Flint and James C. VanderKam; Leiden: Brill, 1998), 430–71 (with references). For a criticism, see Joseph Atwill, Steve Braunheim, and Robert Eisenman, “Redating the Radiocarbon Dating of the Dead Sea Scrolls,” *DSD* 11/2 (2004): 143–73, and the reply by Johannes van der Plicht, “Radiocarbon Dating and the Dead Sea Scrolls: A Comment on ‘Redating,’” *DSD* 14/1 (2007): 77–89.

(109) DJD 18, 96: “early Herodian formal hand”.

(110) The suggested 1 σ calibration published in 1998 is 168–51 BCE.

(111) So, among others, Hempel, *Qumran Rule Texts*, 104–5, 119; Metso, *Textual Development*, 90; Schofield, *From Qumran to the Yahad*, 273–79.

(112) “One Work or Three? A Proposal for Reading 1QS–1QSa–1QSB as a Composite Work,” *DSD* 25/2 (2018): 141–77. See also Johnson’s reply on p. 68 n. 73 to Ben-Dov et al., “Reconstruction of a Single Copy,” who argued against Charlotte Hempel that 1QSa 1:1–3//4QSE I 1–3 is not an “editorial edition”. Cumulative arguments by Ben-Dov et al. actually confirm our view that the Sons of Zadok had not been part of 4QSE but were included only in 1QSa.

a few words—full or partial—which parallel the Zadokite passages in 1QSa: 4Q249^g 1 1–2//1QSa 1:2; 4Q249^e 1 II 1–2//1QSa 1:24. Pfann dated the Cryptic A script of 4Q249^e to the “first half to the mid” 2nd c. BCE, whereas that of 4Q249^g to the “last quarter” of the 2nd c. BCE. Pfann suggests originally these fragments contained reference to the Sons of Zadok as well. (113) This would mean that by the mid-2nd c. BCE there existed an Sa textual tradition that included the Sons of Zadok.

Jonathan Ben-Dov, Daniel S. Ben Ezra and Asaf Gayer in their joint study challenged Pfann’s assignments of certain fragments. (114) They reconstructed 14 out of 23 fragments in five columns (mostly 16 lines per column) of the single 4QSa exemplar, which they labeled as 4Qpap Cryptic A SE (4Q249a). Thus 4Q249^g frg. 1 (DJD) they list as 4QSE (4Q249a) I 2–5, while 4Q249^e 1 II (DJD) as 4QSE (4Q249a) III 6–10. (115) These scholars—like several others—also doubt Cryptic A script can be dated with any precision on the basis of such tiny fragments. They think “[t]he scribe of 4Q249a pap CryptA Serekh haEdah closely resembles and possibly is the same person responsible for 4Q249 Midrash Moshe”, which they date to “ca. 100 BCE”. (116) Their reconstruction has been indirectly confirmed by Émile Puech. (117)

The authors’ reconstruction of 4QSE (4Q249a) I 3 (=4Q249^g 1)//1QSa 1:2 “does not allow the longer text **מִשְׁפַּט בְּנֵי צְדוֹק הַכֹּהֲנִים**” but only **מִשְׁפַּט הַכֹּהֲנִים**]. (118) But in agreement with Pfann, they fill in the missing words in 4QSE (4Q249a) III 7 (=4Q249^e 1 II 1–2) from 1QSa 1:24, which contains the Sons of Zadok. However, the cryptic text may have equally mentioned the Sons of Aaron referred to just before in 4QSE (4Q249a) III 4//1QSa 1:23. This is plausible also in the light of the claimed absence of the Sons of Zadok in 4QSE I 3. The same applies to 4QSE (4Q249a) IV 2, for which the editors rely on 1QSa 2:3, again, without providing a comment. (119)

(113) See DJD 36, 515–74.

(114) Jonathan Ben-Dov, Daniel Stökl Ben Ezra, and Asaf Gayer, “Reconstruction of a Single Copy of the Qumran Cave 4 Cryptic-Script Serekh Haedah,” *RevQ* 29/1 (2017): 21–77.

(115) See the synopsis in “Reconstruction of a Single Copy,” 25–26. 4Q249a (=4QSE) should be distinguished from 4Q249^a to which Pfann, DJD 36, 547–49, assigns two fragments.

(116) Ben-Dov et al., “Reconstruction of a Single Copy,” 31. By contrast, Pfann, DJD 36, 4, posited 4Qpap cryptA Midrash Sefer Moshe should be dated to “no later” than mid-2nd c. BCE.

(117) “Préséance du Messie prêtre,” 85.

(118) Ben-Dov et al., “Reconstruction of a Single Copy,” 39, 62, 67–68.

(119) Ben-Dov et al., “Reconstruction of a Single Copy,” 47–48, 50, 64.

There is thus no secure evidence for the Sons of Zadok in 4QSE. Discussing the content of Cave 4 fragments, Pfann concluded that they witness to “the early history and development” of Sa composition. Even if one reconstructs the missing words in 4QSE by simply following 1QSa, in light of the re-proposed date, one may postulate that the Sons of Zadok had entered the 4QSE text not earlier than ca. 100 BCE.

As already stated, following Józef T. Milik (DJD 1, 107–8), a number of paleographers have repeatedly claimed that the scribe of 1QS wrote other sectarian scrolls, such as 1QSa, 1QSB, 4QTest (4Q175), 4QSam^c (4Q53), and that he made some corrections in 1QIsa^a, and more. (120) As also mentioned, all three texts may have been once part of the same scroll. The usually suggested paleographic date for this scribe is 100–75 BCE, a date supported by C-14 testing.

With regard to 4Qpap pIsa^c, in his notes on the fragments published by Allegro in DJD 5, John Strugnell observed: “[i]l faudrait insister sur l’antiquité de cette écriture; on la daterait de la même période que 1 Q *Serek*, donc au début du 1^{er} siècle avant J.C.”; Strugnell then specifies that its writing is “«semiformel» hasmonéen”. (121) It is worth adding that all Zadokite DSS fall within Tov’s category of “Qumran scribal practice”. (122) He finds the “proportionally largest number” of scribal marks in 1QS–1QSa–1QSB and in 4Qpap pIsa^c.

In the case of 4QFlor, it is a composite text to be dated very probably to the first half of the 1st c. BCE, and which probably draws on an earlier exemplar or separate sources. (123) With regard to 4QFlor, its script was dated by at least some paleographers to early Herodian period. (124)

(120) Even though 4QTest was very probably copied by the same scribe of 1QS–1QSa–1QSB, I not treat it here as its preserved text does not mention the Sons of Zadok. Both John Strugnell, “Notes en Marge du Volume V des «Discoveries in the Judaean Desert of Jordan»,” *RevQ* 7/2 (1970): 225, and Frank Moore Cross, in DSSHAGT 6B, 308, dated paleographically 4QTest to the mid-Hasmonean period, roughly 100–75 BCE.

(121) “Notes en Marge du Volume V,” 188. Strugnell’s view was followed by many authors who refer to 85 BCE as the approximate year (e.g., DJD 39, 390). Tov, *Scribal Practices*, 167, 174, 195, noted the large number of scribal markings in 4Qpap pIsa^c as well as other scribal features attested also in 1QS, 1QSa, 1QSB. Recently, however, Eric D. Reymond, “The Scribe of 1QS, 1QSa, 1QSB, 4Q53 (4QSam^c), 4Q175 and Three Features of Orthography and Phonology,” *DSD* 25/2 (2018): 238–54, has drawn attention to some idiosyncrasies in 1QS when compared to other scrolls.

(122) See further, *Scribal Practices*, 263–73.

(123) Steudel, *Midrasch zur Eschatologie*, 202–10, 215, dates the composition of 4Q174 to 71–63 BCE. However, her suggestion that 4Q174 and 4Q177 are part of the same composition encountered skepticism from such authorities as George J. Brooke and Devorah Dimant.

(124) So already Strugnell, “Notes en Marge,” 177.

Since cumulative elements point roughly to the period when Janneus ruled as high priest and king, we shall look for possible events or situations which may have generated this Zadokite scribal activity.

10. Living under Alexander Janneus, High Priest and King (103–76 BCE)

The important historical question that follows is what might have prompted the Zadokite scribes to revise some of the most important documents of the *yahad*. Internal issues and growing needs within the *yahad* definitely contributed in a significant way to further literary activity. (125) The same applies to the difficult external relations which the *yahad* appears to have experienced with the temple priesthood. (126) Not least, certain ideological tendencies and expressions found in the scrolls likely mirror the perception and experience their authors had of Hasmonean rule.

From the very beginning, the Hasmoneans used various means of legitimizing their controversial high priesthood, both on internal and external levels: political, diplomatic, military, religious, socio-economic, and other. They also produced propagandistic ‘official’ literature such as First Maccabees and the ‘Chronicle’ of Hyrcanus I (cf. 1 Macc 16:24), the aim of which was to legitimize the establishment of the Hellenistic model of Hasmonean high priesthood. (127) These writings were very probably originally composed in Hebrew but were soon after translated into Greek. It is highly probable that, by the time the *yahad* scribes revised their documents, these (and perhaps other) Hasmonean writings were already in circulation. Another means of furthering their propagandistic interests was coinage, which must have widely circulated in Hasmonean Judea. Hyrcanus I was probably the first Hasmonean to strike coins in his own name after 129 BCE, and after him Aristobulus I, but with the title of high priest only. (128) The first Hasmonean to issue coins with both high priestly and royal titles—though

(125) See, for example, Hempel, “The Long Text of the *Serekh*,” 17–20.

(126) See, for example, George J. Brooke, “Crisis Without, Crisis Within: Changes and Developments within the Dead Sea Scrolls Movement,” in *Judaism and Crisis: Crisis as a Catalyst in Jewish Cultural History* (eds. Armin Lange, Diethard K. Römhild, and Matthias Weigold; SIJD 9; Göttingen: Vandenhoeck & Ruprecht, 2011), 89–107.

(127) On this aspect, see further Babota, *The Institution of the Hasmonean High Priesthood*, 10–11, 15, 288–91. On the propagandistic role of 1 Maccabees, see also Edward Dąbrowa, *The Hasmoneans and Their State* (Krakow: Jagiellonian University Press, 2010), 139–43.

(128) Ya’akov Meshorer, *A Treasury of Jewish Coins: From the Persian Period to Bar Kokhba* (Jerusalem: Yad Ben Zvi Press, 2001), 201–209.

never officially with both titles together—was Janneus. (129) Eyal Regev hypothesizes that the usage of both titles on the same coin may have been interpreted as “a vulgar display of power”, “inappropriate”, or “offensive”. (130)

Not only would the Zadokite scribes have been familiar with Janneus’s coinage; they were also very probably aware of the Hasmoneans’ propagandistic writings. Furthermore, if various *yahad* members could neither live ‘unnoticed’ nor remain ‘untouched’ by the policy of Hyrcanus I, this would have been even more true of those who lived under Janneus. By their sheer proximity to Qumran and other places, Hasmonean palaces in Jericho, Masada and elsewhere in the region of the Dead Sea would have been familiar to various *yahad* members. (131) Equally, the *yahad* could hardly ignore the many wars and punitive actions conducted by this ruler in and outside Judea. Some of them must have been eye-witnesses, while others probably had personal experience of at least some of the actions undertaken by this high priest and king.

We have discussed the ideology of two Messiahs as an implied criticism of the combination of civil and religious offices by the Hasmoneans. Already the high priest Hyrcanus I acted as *de facto* king at least in the last years of his rule. Flavius Josephus (37–ca. 100 CE) narrates that Hyrcanus I was reportedly asked by a certain Eleazar, a Pharisee, to give up the high priesthood on the pretext that his mother (i.e., the wife of the high priest Simon) had been a captive under Antiochus (IV) Epiphanes, and instead content himself with ruling the people (*Ant.* 13.288–295). The outcome was Hyrcanus’s break with the

(129) On Janneus, see further Vasile Babota, “Alexander Janneus as High Priest and King: Struggling Between Jewish and Hellenistic Concepts of Rule,” *Religions* 11, 40 (2020): 1–16. While the royal title may appear in Paleo-Hebrew, Aramaic or Greek, the high priestly title is written only in Paleo-Hebrew. With regard to Janneus’s overstruck coins, Yehoshua Zlotnik, “Alexander Jannaeus’ Coins and Their Dates,” 14 [retrieved from www.academia.edu], made the attractive suggestion that “it can be assumed that the ‘priestly’ minting that was done on top of the ‘royal’ minting was not meant to blur or erase the previous minting but to incorporate it in such a way that will display another minting on the coins so both ‘royal’ and ‘priestly’ minting”. See also Meshorer, *Treasury of Jewish Coins*, 37, 209–17; Christian-Georges Schwentzel, “Images du pouvoir et fonctions des souverains hasmonéens,” *RevBib* 163/3 (2009): 368–86. On the coins of Mattathias Antigonus (40–37 BCE) containing both titles, see Meshorer, 218–20.

(130) *The Hasmoneans: Ideology, Archaeology, Identity* (JAJS 10; Göttingen: Vandenhoeck & Ruprecht, 2013), 185–86.

(131) See further Rachel Bar-Nathan, “Qumran and the Hasmonaeans and Herodian Winter Palaces of Jericho: The Implication of the Pottery Finds on the Interpretation of the Settlement at Qumran,” in *Qumran, the Site of the Dead Sea Scrolls: Archaeological Interpretations and Debates* (eds. Katharina Galor, Jean-Baptiste Humbert, and Jürgen Zangenberg; STDJ 57; Leiden: Brill, 2006), 263–77 (with bibliography).

Pharisees and his attachment to the Sadducees. This change reportedly caused much hatred among the masses (13.296–298) to the point that Hyrcanus even had to quiet a revolt (cf. *Ant.* 13.299//*War* 1.68). One also finds mention of a revolt against Hyrcanus I in *War* 1.67. Josephus emphasizes the hatred against Hyrcanus and his sons (*Ant.* 13.296//*War* 1.67). A similar story is reported in *b. Qidd.* 66a, but about Jannai (Janneus). Here, the Pharisees ask Janneus to give up the [high] priesthood “to the seed (לזרע) of Aaron” and keep to himself the royal crown. (132) Both stories end with the split between the Pharisees and the Hasmonean ruler.

Later, in *Ant.* 13.372–383, Josephus recounts how, during a Festival of Tabernacles, the crowd revolted against Janneus and pelted him with citrons and wands made of palm branches (see *m. Sukkah* 4.9 for a similar story). They accused him of being unworthy of high priesthood because of his descent from captives. It could be that Janneus was charged for the same reason as his father, Hyrcanus I. The sedition ended with Janneus reportedly slaughtering 6,000 protesters (*Ant.* 13.373). Josephus also tells that there was a revolt going on against Janneus for six years, during which he killed some 50,000 Jews (*Ant.* 13.376//*War* 1.91). As a result, some Jews asked the Seleucid King Demetrius III Eukerus (97–87 BCE) to intervene and protect them, which he did. Later, during a banquet, Janneus had some 800 of those Jews who defected to Demetrius crucified, while their wives and children were also executed (*Ant.* 13.380//*War* 1.97). (133)

Most historians associate this event with the military campaign of Demetrius against Janneus in 88 BCE. The parallel and somewhat sketchy account in *War* 1.90–98 provides a slightly different version of events. In both versions, Josephus’s source was likely Nicholas of Damascus, the court historian of King Herod the Great, who himself probably drew on an (anti-Janneus?) source.

At least two *pesharim* also appear to have portrayed Janneus negatively. (134) 4QpNah (4Q169) 3–4 I 1–12 appears to have recorded both the military campaign of King Demetrius on the advice of the

(132) Both Günter Stemberger, *Pharisäer, Sadduzäer, Essener* (Stuttgarter Bibelstudien 144; Stuttgart: KBW Bibelwerk, 1991), 100–102, and Emmanuelle Main, “Les Sadducéens vus par Flavius Josèphe,” *RevBib* 97/2 (1990): 199–201, claim that Josephus’s and Talmudic stories refer to Janneus. Other scholars think they refer to Hyrcanus I; so, for instance, James C. VanderKam, *From Joshua to Caiaphas: High Priests after the Exile* (Minneapolis, MN: Fortress Press, 2004), 299–300 (with bibliography). The identity of the two Hasmonean rulers is debated in *b. Ber.* 29a.

(133) These numbers should not be taken literally, of course.

(134) For a discussion of Josephus’ accounts and the DSS on Janneus, see Eshel Hanan, *The Dead Sea Scrolls and the Hasmonean State* (Grand Rapids, MI: W.B. Eerdmans, 2008), 117–31.

“Interpreters of Smooth Things” (דרשי החלקות), most often interpreted as a pun for Pharisees, and their crucifixion by the “Lion of Wrath”, identified by many scholars as Janneus. (135) It speaks of the Lion of Wrath who would “hang men up alive”, which seems to echo the accounts of Josephus.

The same cryptic title of Janneus probably occurs in 4QpHos^b (4Q167) 2 2, used likely interchangeably with the “Last Priest”, “who will stretch out his hand to smite Ephraim”. Ephraim appears in both these *pesharim* and was identified by many scholars as another pun for Pharisees. Both *pesharim* criticize Janneus for the manner in which he punished his enemies, even though their authors did not belong to any of the parties involved. Other DSS may also refer to Janneus, but hardly positively. (136)

The high priesthood alone was not the only target of accusation for opponents of the Hasmonean rulers. It was rather its combination with kingship and especially abuse of both these that seriously bothered opposition groups. According to Josephus (*Ant.* 13.301), Judah Aristobulus I (104–103 BCE) was the first to have proclaimed himself king, but his supposed royal title is unattested elsewhere. However, the first Hasmonean to have effectively and officially acted as both high priest and king for a long time was Janneus (103–76 BCE). Josephus also reports that Hyrcanus I was the first to become rich and hire mercenaries (*War* 1.61//*Ant.* 13.249), but Janneus reportedly had 8,000 (*War* 1.93) or 6,200 (*Ant.* 13.377) of them. In addition to his native soldiers, Janneus used foreign mercenaries against both foreign and domestic enemies, as he was struggling to establish himself as a Hellenistic monarch (so *War* 1.88). (137)

(135) See, among others, Shani L. Berrin (Tzoref), *The Peshar Nahum Scroll from Qumran: An Exegetical Study of 4Q169* (STDJ 53; Leiden: Brill, 2004), 105–106 (with references). However, Gregory L. Doudna, “Allusions to the End of the Hasmonean Dynasty in *Peshar Nahum* (4Q169),” in *The Mermaid and the Partridge: Essays from the Copenhagen Conference on Revising Texts from Cave Four* (eds. George J. Brooke and Jesper Høgenhaven; STDJ 96; Leiden: Brill, 2011), 259, *passim*, thinks the “Lion of Wrath” here stands for “Kittim”, i.e. Romans.

(136) See further Kenneth Atkinson, “Judah Aristobulus and Alexander Janneus in the Dead Sea Scrolls,” *QC* 22/1 (2014): 1–19.

(137) On political achievements of Janneus as well as on his military campaigns and mercenaries, see Chris Seeman, *Rome and Judea in Transition: Hasmonean Relations with the Roman Republic and the Evolution of the High Priesthood* (American University Studies 7/Theology and Religion 325; New York, NY: Peter Lang, 2013), 100–33, 220–26; Katell Berthelot, *In Search of the Promised Land? The Hasmonean Dynasty Between Biblical Models and Hellenistic Diplomacy* (JAJS 24; Göttingen: Vandenhoeck & Ruprecht, 2018), 267–79, 324–40. On Janneus as high priest and king with a view on his internal opposition groups, see VanderKam, *From Joshua to Caiaphas*, 318–36; Kenneth Atkinson, *A History of the Hasmonean State: Josephus and Beyond*

Later, while Pompey stopped in Damascus in 63 BCE, a Judean embassy accused the Hasmonean rulers of instituting a kingdom with the help of mercenaries (Diod. 40.2; Jos., *Ant.* 14.41–47//*War* 1.131–132). Not much later, the author/s of the Psalm of Solomon 17 defended the Davidic kingship by implicitly denouncing that of the Hasmoneans. (138) Chris Seeman noted how, “[i]n both Diodorus and Josephus, the anti-monarchic material associates Hasmonean kingship with violence, whether perpetrated against the nation or its neighbors”. (139) Janneus in particular is portrayed in ancient sources as perhaps the bloodiest Hasmonean ruler. His punitive policy in and outside Judea characterized much of his long rule. The above-preserved accounts—there were probably more—suggest that at least some of the *yahad*’s members could hardly have remained disinterested in these developments.

To be sure, since the Babylonian exile and before Janneus, no one appears to have officially claimed kingship—not counting Aristobulus I, whose rule lasted less than a year and whose royal title is not well documented. With Janneus, kingship becomes an officially established office, as is illustrated by both literary and non-literary sources, especially coins. (140)

One last partly preserved scroll to briefly consider here is the so-called 4QApocryphal Psalm and Prayer (4Q448), paleographically dated towards the mid-first c. BCE. It preserves portions of Ps 154 (known also from 11QPs^a) and a Prayer (?) which in col. 2:2 reads כ על יונתן המל. There are several interpretative problems with this document as a whole. Some scholars theorized it once formed the introduction to MMT. Others identified the mentioned “Jonathan” either with the first Hasmonean high priest or with Janneus. (141) Still others questioned

(JCTCRS 23; London: Bloomsbury T&T Clark, 2016), 100–33. On the enemies of Janneus, see also Benedikt Eckhardt, “The Hasmoneans and Their Rivals in Seleucid and Post-Seleucid Judea,” *JSJ* 47/1 (2016): 66–70. By contrast, Jewish mercenaries are known to have fought on behalf of Persian (see the Elephantine community) and Ptolemaic (see the Leontopolis community) sovereigns in Egypt.

(138) See further Kenneth Atkinson, *I Cried to the Lord: A Study of the Psalms of Solomon’s Historical Background and Social Setting* (JSJSup 84; Leiden: Brill, 2004), 130–79.

(139) On Judean embassy to Pompey, see Seeman, *Rome and Judea*, 256–66 esp. p. 266.

(140) On various sources, see Atkinson, *A History of the Hasmonean State*, 101–104.

(141) In favor of first Jonathan, see among others, Geza Vermes, “The So-Called King Jonathan Fragment (4Q448),” *JJS* 44/2 (1993): 294–300; Émile Puech, “Jonathan le prêtre impie et les débuts de la communauté de Qumrân. 4QJonathan (4Q523) et 4QPs Ap (4Q448),” *RevQ* 17/4 (1996): 241–70; Corrado Martone, “Un inno di Qumran dedicato a ‘Re Gionata’,” *Hen* 19/1 (1997): 131–41; Babota, *The Institution of the Hasmonean High Priesthood*, 165–66 (as a possibility). In favor of Janneus who

whether this text should be considered ‘sectarian’ at all. Also, since only a portion of it survives, it is not clear whether על should be understood as “for” or “against”. If it is a petition “against” Jonathan, then ideologically it would align itself with other known anti-Hasmonean documents. (142) There are also other difficulties posed by this text which further complicate a straightforward interpretation. (143)

Summary Conclusions

The re-emergence of the Sons of Zadok in several DSS, apparently all produced by the *yahad* scribes, has generated a variety of scholarly interpretations. Many scholars have looked at the Sons of Zadok mainly in relation to the Sons of Aaron and most often within the setting of the *yahad*. This study proposes to look at the Sons of Zadok against the backdrop of Hasmonean rule and interpret this label in correlation with other ideologies imported from the Book of Ezekiel, where they appear for the first time. Although perhaps all these DSS are composite, when studying the Sons of Zadok, it is suggested to look at these writings as a corpus.

Apparently the same scribe of 1QS–1QSa–1QSB copied or even inserted seven out of twelve references to the Sons of Zadok. This appellation entered the textual tradition of other compositions, namely CD-A, D^a, 4Qpap pIsa^c, and 4QFlor, probably about the same time as part of the larger Zadokite revision of earlier documents. The lack of the Sons of Zadok in later exemplars or other *yahad* compositions could reflect the aftermath following death of Janneus. This gave rise to a new ruling model under his widow, Queen Salome Alexandra:

appears on his coins as יהונתן, יונתן, and probably ינתן, see among others, André Lemaire, “Le roi Jonathan à Qoumrân (4Q448, B–C),” in *Qoumrân et les manuscrits de la mer Morte. Un cinquantenaire* (ed. Ernest-Marie Laperrousaz; Paris: Cerf, 1997), 57–70 (first Jonathan as a possibility); Esther Eshel, Hanan Eshel, and Ada Yardeni, “4Q448. 4QApocryphal Psalm and Prayer,” in *Qumran Cave 4, VI: Poetical and Liturgical Texts, Part 1* (DJD 11; Oxford: Clarendon Press, 1998), 403–25, who stress the non ‘sectarian’ nature of 4Q448 as incompatible with other known anti-Hasmonean documents (p. 415); so also VanderKam, *From Joshua to Caiaphas*, 336.

(142) So Emmanuelle Main, “For King Jonathan or Against? The Use of the Bible in 4Q448,” in *Biblical Perspectives: Early Use and Interpretation of the Bible in Light of the Dead Sea Scrolls. Proceedings of the First International Symposium of the Orion Center for the Study of the Dead Sea Scrolls and Associated Literature, 12–14 May, 1996* (eds. Michael E. Stone and Esther G. Chazon; STDJ 28; Leiden: Brill, 1998), 113–35, who concludes that “[i]t is not a prayer on behalf of ‘King Jonathan’ but a wish that God Himself will fight against him” (p. 135).

(143) Tov, *Scribal Practices*, 284, and esp. 342, finds no orthographic or morphological features in 4Q448 characteristic of what he defines as “Qumran scribal practice”.

namely, the re-separation of high priestly and royal offices. Besides, a new 'enemy' appeared on the horizon: the "Kittim", mentioned in several *pesharim* and other *yaḥad* DSS, and very probably referring to Romans.

The joining of the *yaḥad* by new members—both priests and non-priests—probably took place periodically, especially since the latter years of the rule of John Hyrcanus I. However, as a result of the controversial actions of Alexander Janneus in and outside Judea, it is expected that the number of *yaḥad* members would have continued to increase. In this sense, the Zadokite scribal activity could be partly connected with such 'exile', which may have even coincided with the settlement at Qumran. Yet this study does not support the view of a 'Zadokite takeover' of the *yaḥad* leadership. It is argued that rather a major self-reassertion of the now expanded *yaḥad*—or at least of a significant part of it—took place.

The Book of Ezekiel, with its future-oriented ideologies, played a significant role in this process, which—perhaps more than any other biblical text—influenced the scribal revision of several important documents of the *yaḥad*. Similarly, as in Ezek 40–48, in the DSS the Sons of Zadok are used ideologically. Unlike the Sons of Aaron, the Sons of Zadok in the DSS appear almost exclusively in the *yaḥad* setting, in contexts which speak of "the last days" and, in several cases, where an eschatological temple is foreseen. Unlike in Ezek 40–48, in the DSS this designation can at times also include non-priests as part of the self-characterization of the *yaḥad*. Such usage may have been an ideological strategy by the leadership of the *yaḥad* for elevating the status also of its non-priestly members. It may also explain why the Sons of Zadok are associated more with administrative tasks when compared to the Sons of Aaron, which was the official designation for Second Temple priests.

Most importantly, the Sons of Zadok appear in contexts which propagate—in one way or another—the coming of two Messiahs: a priestly one and a royal one, a few times meaning a Davidic figure. In fact, the ideological themes developed in the Zadokite DSS strongly suggest that their authors were deeply concerned about central institutions, namely the temple, the (high) priesthood, and the kingship. They promote a new (eschatological) ruling model which can be interpreted as an implicit criticism of the combination of high priesthood with kingship in one person. Ben Sira 51:12a–o too may have been produced by the same Zadokite scribes or by a (pro-Oniad?) group as it shared similar anti-Hasmonean views, just as did later the authors of the PssSol. These ideologies can be confidently read as a response to perceived abuses of these two offices, especially by Janneus. It is

also likely that these ideologies are an implicit response to the propagandistic claims made by the Hasmoneans in their 'official' documents such as 1 Maccabees, the 'Chronicle' of Hyrcanus I, and perhaps others.

Therefore, it is proposed to interpret the Sons of Zadok as coinciding with the emergence of the ideology of two Messiahs. It was yet another way to validate the institutional model which stipulates separation of the high priestly office from the civil one. Thus, the adoption of the designation "Sons of Zadok" in the DSS can be understood as yet another implicit criticism directed at Janneus. More specifically, it can be perceived as a rejection of his high priesthood and of the Jerusalem temple priesthood, in general. Not least, this ideological label can be interpreted as a further radicalization and self-perception of the *yahad* members, or at least of a significant part of them.

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ADDITIONAL 4Q397 (4QMMT^D) FRAGMENTS

AFTER the publication of 4Q397 (frags. 1–23) by the editors of 4QMMT, (1) I identified two more fragments of this manuscript: PAM 43.398 frag. 7 (4Q397 frag. 24) (2) and PAM 43.668 frag. 4 (4Q397 frag. 25). (3) Emile Puech numbered a fragment which was included in the transcription of 4Q397 frags. 6–13 (lines 7–8), but not placed on the edition's plate, as 4Q397 frag. 26, and put it eight lines higher in the column than the editors did. (4) Based on the identification of frag. 25, which preserves part of the right margin, Puech reconstructed the last three columns of the manuscripts with eighteen lines per column, and tentatively placed the remaining fragments in three other columns, proposing that this manuscript of MMT consisted of six columns in total. (5) His proposal is accompanied by transcriptions as well as drawn figures of this reconstruction. The present note proposes to assign two more hitherto unidentified fragments to 4Q397 and compares their possible placement to Puech's reconstruction. (6)

(1) Elisha Qimron and John Strugnell, *Miqṣat Ma'āse ha-Torah*, DJD 10 (Oxford: Clarendon, 1994).

(2) Eibert Tigchelaar, "Publication of PAM 43.398 (IAA #202) Including New Fragments of 4Q269," in *From 4QMMT to Resurrection: Mélanges qumraniens en hommage à Émile Puech*, ed. Florentino García Martínez, Annette Steudel, and Eibert Tigchelaar; STDJ 61 (Leiden: Brill, 2006), 265–80, at 269.

(3) Eibert Tigchelaar, "PAM 43.668 Frag. 4 Identified as a 4Q397 (4QMMT^d) Fragment," *RevQ* 26/103 (2014): 455–59.

(4) Emile Puech, "La *Lettre* essénienne *MMT* dans le manuscrit 4Q397 et les parallèles," *RevQ* 27/105 (2015): 99–135, esp. 108–10.

(5) Puech, "4Q397 et les parallèles."

(6) I have made use of images which were provided by the The Leon Levy Dead Sea Scrolls Digital Library; Israel Antiquities Authority to the ERC project of the European Research Council (EU Horizon 2020): The Hands that Wrote the Bible: Digital Palaeography and Scribal Culture of the Dead Sea Scrolls (HandsandBible #640497), principal investigator Mladen Popović. Publication of sections of these photographs are

1. PAM 43.680 frag. 16 (4Q397 frag. 27)

The editors of the volume of Cave 4 unidentified fragments could make little sense of the traces, but an enlargement of the image of PAM 43.680 frag. 16 clearly shows that the traces of its second line should be read]ש[א[ן]זמה רואים (7). In the corpus of Classical Hebrew, the expression]ש[א[ן]זמה רואים is only preserved in 4QMMT (4Q394 8 iv 1; MMT 50). Though only the few letters of line 2 remain on PAM 43.680 frag. 16, palaeographically they are compatible with no more than one MMT manuscript, namely 4Q397. Its full spelling (*waw* in רואים) and long third person plural pronominal suffix are also in agreement with the orthographic practice of 4Q397 (8) which Emanuel Tov refers to as the Qumran Scribal Practice. (9) The one palaeographical variance between the letters of this fragment and those of 4Q397 is its relatively long final *mem*, extending below the virtual base line, while the final *mems* in 4Q397 are characteristically squat, often resting on the virtual base line. It is generally acknowledged that the scribe of 4Q397 wrote several other manuscripts in a so-called round semiformal script. (10) John Strugnell described the squat final *mem* as typical for this specific semiformal series, but longer final *mems* are attested in some manuscripts penned by the scribe of 4Q397, as in 4Q227. (11) Palaeographically,

by courtesy of The Leon Levy Dead Sea Scrolls Digital Library. I thank Gemma Hayes for commenting on an earlier version. The author is also a research associate of the University of Pretoria.

(7) Publication in Dana M. Pike and Andrew C. Skinner, *Qumran Cave 4, XXIII: Unidentified Fragments*, DJD 33 (Oxford: Clarendon, 2001), 151, Pl. XXI. I have consulted the photograph PAM 43.680 (<https://www.deadseascrolls.org.il/explore-the-archive/image/B-285459>), the new color photograph of Museum Plate 71, and the new infrared image of the fragment (courtesy IAA and the ERC project). Presently, the piece with the *alef* of]ש[א[ן]זמה רואים has broken off and is no longer located on the same museum plate,

(8) Cf. the long form in 4Q397 1–2 2; 5 2 and 4; possible 4Q397 6–13 10 זמה; but the short form in 4Q397 6–13 8 and 10 להאכלם.

(9) Cf. Emanuel Tov, *Scribal Practices and Approaches Reflected in the Texts Found in the Judean Desert*, STDJ 54 (Leiden: Brill, 2004), 337–38. On the orthography of the scribe of 4Q397 and that of the other MMT manuscripts, cf. the forthcoming publications by Gemma Hayes (PhD student at the University of Groningen in the ERC project “The Hands that Wrote the Bible”).

(10) See Ada Yardeni, “A Note on a Qumran Scribe,” in *New Seals and Inscriptions: Hebrew, Idumean and Cuneiform*, ed. Meir Lubetski; HBM 8 (Sheffield: Phoenix, 2007), 287–98; my 2013 conference paper “Scribal Practices As Attested in the Dead Sea Scrolls Manuscripts Written in a So-Called Rustic Semi-Formal Tradition” (<https://doi.org/10.5281/zenodo.1925681>); and the present PhD work of Gemma Hayes.

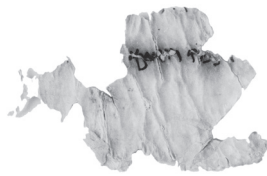
(11) John Strugnell, “375. 4QApocryphon of Moses,” in *Qumran Cave 4, XIV: Parabiblical Texts, Part 2*, ed. Magen Broshi et al., DJD 19 (Oxford: Clarendon, 1995), 111–19, at 112. 4Q227 (4QpsJub⁶), a manuscript which has been ascribed to the scribe of 4Q397 by both Yardeni, “Note,” 289 and Hayes (forthcoming work), has both squat

the longer final *mem* is unique for 4Q397, but it falls in the variational range of final *mems* of this scribe. One might hypothesize that its longer form is related to the large empty area below the line.

In the infrared photographs, the fragment seems to have a large bottom margin of 2.1 cm. The new color image of Plate 71 shows two horizontal ruling lines in this uninscribed area of the fragment. This could indicate that the fragment does not derive from the bottom of the sheet, but left two lines uninscribed. However, several scrolls, e.g., 4Q41 (4QDeutⁿ), do have ruling lines down to the bottom of the sheet (cf. most clearly in col. 2). (12)

Strong support for regarding this fragment as a 4Q397 fragment with a bottom margin is provided by Puech's reconstruction of the scroll. In his drawing of 4Q397 col. II the words **שאינם רואים** (Puech chose to reconstruct **שאינם** rather than **שאינמה**), appear at the very bottom of col. II, one line lower than the bottom of 4Q397 frag. 5. (13) The three small traces of the first line do not perfectly fit with Puech's drawing, but this is because it is impossible to reconstruct the exact size and position of the margins and the blanks in a column like this. The remains of the first line possibly fit with *reš* and *'alef* of **ראוי**, which would perfectly fit if the scribe did not delineate sections with a blank space in the bottom line. The fragment should then be placed somewhat to the right of frag. 5. (14)

If we regard this fragment as a 4Q397 fragment in spite of the longer final *mem*, then it is the first 4Q397 fragment with a top or bottom margin, and it confirms Puech's overall reconstruction of the scroll which was not aided by the presence of top and bottom margins on the preserved fragments.



final *mem* and several cases of longer final *mem* in frag. 2. Cf. especially line 3 **כולם** (personal communication of Gemma Hayes).

(12) Tov, *Scribal Practices*, 56, who gives more examples.

(13) Puech, "4Q397 et les parallèles," 113.

(14) There is one more small trace at the left of the bottom line, which may be part of the downstroke of *reš* of **להזהר**.

2. PAM 43.694 frag. 51 (4Q397 frag. 28)

The evidence linking PAM 43.694 frag. 51 to 4Q397 is somewhat more limited. The fragment only preserves the end of two lines and has a large piece of uninscribed leather. In line 1 one should read תַּשְׁמָה, (15) and in the second line תַּ. The fragment might be placed immediately to the left of 4Q397 frag. 23 (see Puech's figure 1) so that it preserves the end of col. VI line 3 שתַּשְׁמָה, and of line 4 מַקְצַת. This would mean that the word דְּבָרֵינוּ, which Puech reads at the end of line 4 after מַקְצַת would have been written in the next line, while each of the next lines would be slightly narrower than assumed by Puech. If the join is valid, frags. 23 and 28 physically join and together preserve the *taw* of תַּשְׁמָה. However, in the next line part of the leather between frag. 23 מַקְצַת and frag. 28 the left part of *taw* has been lost, so that materially the fragments only join at one point.

One may raise a few challenges to the identification. First, as presented on the photograph PAM 43.694, frag. 28 appears materially different from frag. 23. However, the same would go for 4Q397 frag. 27, so that we may surmise that the physical appearance on the photographs is not fully indicative. Second, palaeographically, the *šin* of תַּשְׁמָה is written less curvedly and with a shorter left downstroke than the preceding forms of *šin* in בַּשְׁלֵשׁ in frag. 23. However, these forms of *šin/šin* belong to the variation within 4Q397. (16)

3. Conclusion

Both newly identified fragments provide new evidence in the form of margins about the format of 4Q397. Overall, they closely match the reconstruction of Puech of the respective columns, (17) but the details show that it is virtually impossible to exactly predict the alignment of words on a line.



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(15) The editors, Pike and Skinner, DJD 33, 253 incorrectly read תַּשְׁמָה.

(16) For example, the two forms of *šin* in 4Q397 frag. 5 are similar to the one in frag. 28.

(17) But cf. also some my remarks in "Material Construction and Palaeographic Dating of MMT: the Evidence of the Manuscripts," in *Qumran 4QMMT: Some Precepts of the Law*, ed. Reinhard G. Kratz; SAPERE (Tübingen: Mohr Siebeck, forthcoming).

FOUR UNIDENTIFIED FRAGMENTS FROM 4QJOB^A (4Q99) ⁽¹⁾

Background

IN 2000, Eugene Ulrich and Sarianna Metso published their reconstruction of 4QJob^a (4Q99) in DJD XVI. (2) They identified 19 of the 23 fragments that had been assigned to 4QJob^a in the Palestine Archaeological Museum (PAM) photographs. (3) Of the remaining four, they suggested that one, frag. 23, had likely been misclassified and belonged to a different manuscript. (4) Ulrich and Metso were unable to identify frags. 20–22, (5) although they proposed some tentative readings, which we discuss below. Using the substantial photographic evidence now available, we suggest several new readings and identifications. (6)

(1) We are indebted to Eibert Tigchelaar for his invaluable comments and for bringing to our attention the misidentified fragment from 4QS^b (discussed below), as well as PAM 43.161. We are also grateful to the anonymous reviewers for their insightful comments, which have improved this article.

(2) Eugene Ulrich, Frank Moore Cross, et al., *Qumran Cave 4.XI: Psalms to Chronicles*, DJD XVI (Oxford: Clarendon, 2000), 171–178; see also Eugene Ulrich, Sarianna Metso, “A Preliminary Edition of 4QJob^a,” in *Antikes Judentum und Frühes Christentum: Festschrift für Hartmut Stegemann zum 65. Geburtstag* (ed. Bernd Kollmann, Wolfgang Reinhold, and Annette Steudel; Berlin: Walter de Gruyter, 1999), 29–38.

(3) PAM 41.294, PAM 41.786, PAM 42.638, PAM 43.096.

(4) DJD XVI, 171.

(5) DJD XVI, 178. Ulrich and Metso apparently worked with only Mus. Inv. 1116 and did not see images of these three fragments (*ibid.*, 171).

(6) Namely one additional PAM photograph (PAM 43.161, which contains all three fragments), and several new IAA images from 2013 (especially IAA B-368403, B-368404, B-368405, B-368406, B-368407, B-368408, B-368409, and B-368410).

4QJob^a Fragment 20

Fragment 20 contains five letters, two of which are complete. Ulrich and Metso read]אם לחס[(7) and this reading can be confirmed from the infrared and full-spectrum photographs taken in 1959 and 2013. (8)



4QJob^a frag. 20 (B-368406)

*Courtesy of The Leon Levy Dead Sea Scrolls Digital Library,
Israel Antiquities Authority
Photo: Shai Halevi*

Ulrich and Metso did not propose a specific identification within the book of Job. However, 37:13 appears to be a perfect match:

4QJob^a frag. 20 Job 37:13 (MT)

]אם לשבט אם לארצו אם לחסד ימצאהו [אם לחס]

This attribution places frag. 20 just before frag. 19, which Ulrich and Metso identify as containing parts of Job 37:14–15.

4QJob^a Fragment 21

This fragment preserves portions of two lines. Without identifying the passage to which the fragment corresponds, Ulrich and Metso proposed the following reading: (9)

1]מים ב[

2]ש[

With the aid of the infrared and full-spectrum photographs, it is possible to improve upon this reading and to identify the passage it reflects. (10)

(7) Ibid., 178.

(8) PAM 43.161 (infrared); IAA B-368405 (full spectrum), IAA B-368406 (infrared).

(9) DJD XVI, 178.

(10) PAM 43.161 (infrared); IAA B-368407 (full spectrum), B-368408 (infrared).

4QJob^a frag. 21 (B-368408)

*Courtesy of The Leon Levy Dead Sea Scrolls Digital Library,
Israel Antiquities Authority*

Photo: Shai Halevi

Line 1 indeed reads]^ומים ב^ו], as suggested by Ulrich and Metso, but the ink traces before and after the legible letters limit the possible correspondences. Job 37:17 is the salient candidate:

4QJob^a frag. 21

Job 37:17 (MT)

[חמים בה]שקט

אֲשֶׁר בְּגִידֶיךָ חָמִים בְּהֶשְׁקַט אֶרֶץ מִדְּרוֹם 1

This fragment thus contains material that is a mere two verses removed from that of frag. 19, which, in turn, contains text adjacent to frag. 20.

The reconstruction of line 2 in DJD XVI is]ש^ו]. This is unlikely for several reasons. First, it does not match any known text in the subsequent verses. While substantial divergences from MT are possible, they are the exception, rather than the rule. Moreover, the extant ink traces are a poor match for a *shin*, considering that the left-most stroke of that letter in 4QJob^a invariably runs down and to the right, which is not the case here. Indeed, it appears that the three small traces of ink are the upper remnants of three, not two, letters. The trace to the far right, which is the smallest of the three, corresponds to any of a number of letters. The middle letter is tightly wedged between the two others, making a *yod* or *waw* probable, with *yod* being the best match. (11) The leftmost letter could either be the full upper portion of a *resh*, or else the top-right section of a final *mem*. (12)

(11) For similar *yods*, see, e.g., B-368374, line 3: מאמצי; B-368382, line 4: יאמר; B-368394, line 3: יכלו.

(12) For a comparable final *mem*, see B-368400, line 2: שם[נפ]; B-368398: בנעמים. The right-hand ink trace would then be the uppermost remnant of a *qof*. The relative heights of our conjectured *qof* and *yod* are comparable to those of, e.g., B-368371, line 3: וקנים.

Considering the continuation of the text in MT, the most likely reading would appear to be קִיִּם. This sequence of letters is found twice in 37:18: in לשחקים and חזקים. The former of these two is the more probable candidate, given this manuscript's overall stichographic arrangement. The versification of MT generally corresponds to line breaks in 4QJob^a, and if this is also the case here, לשחקים would be expected to appear beneath חמים בהשקט.

4QJob^a frag. 21

Job 37:17–18 (MT)

[חמים בהשקט]
[לשחקים]

1 אֲשֶׁר בְּגִדֶיךָ תַּמִּים בְּהִשָּׁקֵט אֶרֶץ מְדֻרָּה
2 תִּרְקִיעַ עֲמוּ לִשְׁחָקִים חֲזָקִים כִּרְאִי מוֹצֵק

4QJob^a Fragment 22

Ulrich and Metso proposed the following reading: (13)

1]נפֿעַ עֻּוּ[
2]לִוּוּ[

From the IAA's recent photographs, (14) another reading of the first line presents itself, and we can also improve somewhat on the reconstruction of line 2.



4QJob^a frag. 22 (B-368410)

*Courtesy of The Leon Levy Dead Sea Scrolls Digital Library,
Israel Antiquities Authority*

Photo: Shai Halevi

(13) DJD XVI, 178. In their comments, they noted that the third letter of line 1 might be a *shin*, that the second letter of line 2 might be a *yod*, and that the letter following the final *vav* could be a *dalet*, *he*, *vav*, *het*, *yod*, *kaph*, *mem*, or *pe*. They also expressed doubt regarding the space between the third and fourth letters of line 1.

(14) IAA B-368409 (full spectrum), B-368410 (infrared). This fragment was mounted and photographed upside down.

It seems this fragment contains text corresponding to Job 37:14–15. The first line clearly reads נִפְלְאוֹת. As suggested by Ulrich and Metso, the second line contains the ascender of a lamed, followed by a partial *vav*. The fragmentary third letter appears to be a *he*.

4QJob^a frag. 22

Job 37:14–15 (MT)

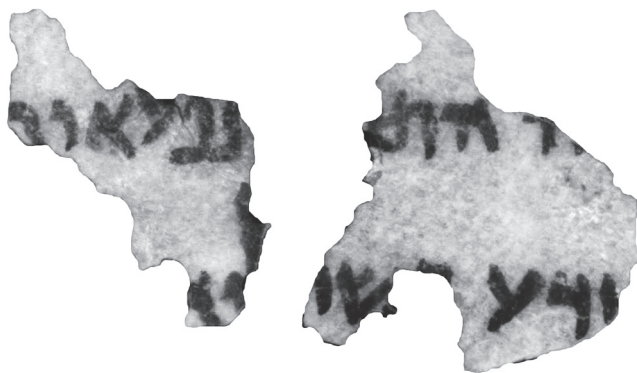
]נִפְלְאוֹת[1

הָאֵינָהּ זֹאת אֵיב עָמַד וְהִתְבּוֹנֵן נִפְלְאוֹת אֵל

]א[לוֹהֶ] 2

הַתְדַע בְּשׁוּם אֱלֹהֶ עֲלֵיהֶם וְהוֹפִיעַ אֹר עֲנָנוּ

This fragment was originally located just to the left of frag. 19, which also contains portions of verses 14–15:

4QJob^a frags. 22 (B-368410) and 19 (B-368404)

Courtesy of The Leon Levy Dead Sea Scrolls Digital Library,

Israel Antiquities Authority

Photos: Shai Halevi

4QJob^a frags. 19+22

Job 37:14–15 (MT)

]נִפְלְאוֹת[1

הָאֵינָהּ זֹאת אֵיב עָמַד וְהִתְבּוֹנֵן נִפְלְאוֹת אֵל

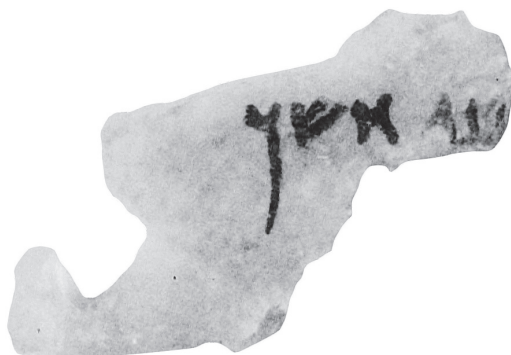
]א[לוֹהֶ] 2

הַתְדַע בְּשׁוּם אֱלֹהֶ עֲלֵיהֶם וְהוֹפִיעַ אֹר עֲנָנוּ

The layout of lines 1 and 2 in these fragments is somewhat unexpected. In MT, no words appear between נִפְלְאוֹת אֵל at the end of verse 14 and הַתְדַע בְּשׁוּם אֱלֹהֶ עֲלֵיהֶם at the beginning of verse 15. Given the manuscript's overall stichographic arrangement and the fact that the latter phrase appears directly under the former in frags. 19 and 22, it is likely that line 1 began with the word עָמַד and line 2 with הַתְדַע. If this is the case, this raises the question of where the first words of verse 14 in MT—הָאֵינָהּ זֹאת אֵיב—could have appeared. Idiosyncratic arrangement is a possibility, but it is also plausible that the text of 4QJob^a differed here somewhat from that of MT.

4QS^b Fragment 1

In PAM 42.372 from 1957, this fragment appears together with fragments from 4QS^b (4Q256), and in DJD XXVI, Alexander and Vermes included it in their edition of the same Serekh manuscript, suggesting the reading [ולשנא כול] בני חשך. (15) They expressed, however, “serious doubts” regarding the attribution, noting irregularities in terms of handwriting and orthography. (16)



4QS^b frag. 1 (PAM 43.698; IAA B-285123)

*Courtesy of The Leon Levy Dead Sea Scrolls Digital Library,
Israel Antiquities Authority
Photo: Najib Anton Albina*

These doubts were well placed. This fragment appears to belong to 4QJob^a, and it contains text from Job 37:19:

4QS ^b frag. 1	Job 37:19 (MT)
[מ]פני חשך	1 הוֹדִיעֵנוּ מָה נֹאמַר לוֹ לֹא נִעְרַף מִפְּנֵי חֹשֶׁךְ
[vacat]	2

The handwriting in this fragment neatly matches that of 4QJob^a. The orthography accords, as well; 4QJob^a is written considerably more defectively than 4QS^b. The bottom portion of the fragment is blank, and it is probable that the column ended here. (17)

(15) Philip Alexander and Géza Vermes, *Qumran Cave 4.XIX: 4QSerekh Ha-Yahad*, DJD XXVI (Oxford: Clarendon, 1998), 40, 47. In PAM 43.698, it appears alongside assorted unidentified fragments.

(16) DJD XXVI, 47.

(17) According to the reconstruction of Ulrich and Metso, the second column of frag. 16 concludes with Job 37:5, which suggests that the following column begins with 37:6. Considering that the manuscript's columns appear to contain sixteen lines each

We propose referring to this fragment in future publications as 4QJob^a frag. 24.

Column Reconstruction

In light of all the above, our tentative reconstruction of the section in question is as follows:

[..... אִם לַחֹסֶד] 10	f.20
[.....] 11	f.19 f.22
[.....] 12	f.19 f.22
[.....] 13	
[.....] 14	f.21
[.....] 15	f.21
[.....] 16	4Q256 f.1
<i>bottom margin</i>	

Conclusion

Frgs. 20–22 of 4QJob^a and frag. 1 of 4QS^b have not before been securely identified. It now appears that all four fragments derive from the lower portion of a single column of 4QJob^a. (18)

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and that single verses (in MT) are occasionally split between two lines, we would indeed expect the column to end here or nearby. (See DJD XVI, 171, 176.)

(18) As is the case for the adjacent columns, the upper portion fared less well. (See DJD XVI, 174–176.)

4Q99 (4QJOB^A) FRAG. 23 (JOB 36:20–22)

IN their edition of 4Q99 (4QJob^a), (1) Eugene Ulrich and Sarianna Metso included four unidentified fragments (frags. 20–23), three of which (frags. 20–22) have been identified by Rebekka Luther and Idan Dershowitz as deriving from Job 37, who also fitted 4Q256 frag. 1 in Job 37. (2) About the last remaining unidentified fragment, frag. 23, not discussed by Luther and Dershowitz, the editors argued,

This fragment quite probably does not belong to 4QJob^a: except for the final *mem*, the letters are not formed like those of 4QJob^a, and ענפֿים does not occur in Job. (3)

This raises the question why, from the very first collection of 4QJob fragments onwards, the arrangement of 4Q99 included a fragment which—according to the editors—would not fit either textually to Job or palaeographically to 4QJob^a. The fragment is included in PAM 41.294, 41.786, and 43.096. In the first two of these photographs, the fragment is placed at the bottom of the plate, beneath other 4Q99 fragments which were ordered by the MT textual sequence. However, in PAM 43.096, which contains 4Q99 frags. 1–19 and frag. 23, arranged according to the textual order of the book of Job, frag. 23 has been placed in between

(1) Eugene Ulrich and Sarianna Metso, “99. 4QJob^a,” in *Qumran Cave 4. XI: Psalms to Chronicles*, ed. Eugene Ulrich et al.; Discoveries in the Judaean Desert XVI (Oxford: Clarendon, 2000), 171–78. Henceforth: DJD XVI. For important improvements on the reading of frags. 4 1–2 and 16 ii 8–11, cf. Emile Puech, “Glanures épigraphiques: le livre des Proverbes et le livre de Job à Qumrân,” in *Textual Criticism and Dead Sea Scrolls Studies in Honour of Julio Trebolle Barrera: Florilegium Complutense*, ed. Andrés Piquer Otero and Pablo A. Torijano Morales; JSJSup 157 (Leiden: Brill, 2012), 277–302, esp. 285–87.

(2) Rebekka Luther and Idan Dershowitz, “Four Unidentified Fragments from 4QJob^a (4Q99).” I wish to thank both authors for sharing their work with me, and for stimulating discussions on the variants in this fragment.

(3) DJD XVI, 178.

frag. 5 (Job 33:30) and frag. 6 (Job 35:16), suggesting that according to Frank Moore Cross, originally responsible for 4Q99, frag. 23 possibly corresponded to a section in between Job 33:30 and 35:16.

1. Reading the Fragment

Ulrich and Metso commented that, “except for the final *mem*,” the letters of the fragment were “not formed like those of 4QJob^a.” However, not only the final *mem*, but also the *taw* and the *yod* before final *mem* are formed like those of 4QJob^a. The same holds for the form of *ayin*, though the right stroke is shorter than in any of the exemplars in the other 4QJob^a fragments. The editors’ palaeographic verdict has probably been influenced by their interpretation of the second and third letters of the only completely preserved word, which they read as ענפֿים. Both letters are strange. The second letter has the head of a *waw/yod* and the basestroke of a *nun*. The third letter has the form of a *nun*, except for a large head which is commonly found with *pe/yod*. In this letter, however, headstroke and downstroke are formed by two separate strokes, unlike the usual writing of *yod* or *pe*. All these oddities result from the scribal correction of עינים to עניים, namely by adding a basestroke to the second letter, *yod*, and by adding a separate large head to the third letter, *nun*. (4)

The first line of 4Q99 frag. 23 should therefore be read after its correction from עינים to עניים as:

1 [עניים ת]

2. Failed Textual Identifications

The sequence of letters of frag. 23 line 1 does not occur in MT Job, but given the arrangement of the fragments on PAM 43.096 it is possible that Cross tentatively associated the reading with Job 34:28b which reads וַיִּשְׁמַע יְהוָה בְּצִיָּקוֹת צָרִים וְשֹׁמְרֵי חֻמֹּתָם, “and he heard the cry of the afflicted.” This tentative identification of frag. 23 also seems to be implied in the two indices to the DJD volume, which, though not supported by the edition itself, refer to Job 34:28–31 being attested in 4QJob^a. (5) Undoubtedly,

(4) That עינים was first written and then subsequently corrected to עניים, and not the other way around, does not only appear from the strokes, but also from the spacing between the letters. For other corrections in 4Q99, see 7 ii, 12–16 i line 16 where *taw* of תשניא (thus also MT) has been erased, and 16 ii 11 the word וּבְהִנָּה (for the reading see Puech, “Glanures épigraphiques,” 287).

(5) DJD XVI, 300 and 302. For the same identification, cf. already G. Wilhelm Nebe, “Qumranica I: Zu unveröffentlichten Handschriften aus Qumran Höhle 4,”

the reference to Job 34:28ff goes back to the reading of ענייִם in line 1, rather than to the incorrect reading ענפִים of the editors.

Nonetheless, the identification with Job 34:28–31 should be rejected. The other traces on the fragment are all difficult or impossible to match to the text of MT Job 34:28ff. The first letter of the line does not fit *qop*, and at the end we have to read a word with *taw*, rather than ישמע. Line 2 seems to have the two last letters of a hemistich, ending, most likely in *zayin* or final *nun*, which again does not fit MT Job 34:28ff. In the third line the editors transcribe]לָל[, even though the distance between the two downstrokes is fairly minimal for two consecutive *lameds*. It is therefore in none of the lines of frag. 23 possible to propose a reading and reconstruction which remains related to both the text of MT Job 34:28ff and the layout of 4QJob^a. Only ענייִם preceded by ת fits with Job 34:28ff, where all the other evidence diverges.

If one searches more broadly in other texts for a textual overlap with the traces of line 1, only Ps 9:19 serves as a possible parallel. One might then read תַּקְלוֹת עֲנִיִּים תִּאֲבָד. However, it is impossible to match the traces of lines 2 and 3 with the end of Ps 9, or the beginning of Ps 10.

3. A Material-Textual Suggestion

Since the fragment seems to correspond materially and palaeographically to 4Q99, but none of the readings ענפִים (so the editors), עֲנִיִּים (first hand) or עֲנִיִּים (scribal correction) fit with the MT text of Job, one may pursue a material-textual approach: can one find a place in 4Q99 where this fragment could fit better materially and textually, even without corresponding entirely to MT? Given the other variants in 4Q99, the fragment may have a somewhat different text from MT, while nonetheless its remaining textual data as well as the material features of the fragment may suggest a specific placement in 4Q99.

The supposition is that the word which is corrected in the manuscript, and does not fit with MT Job, may reflect a variant vis-à-vis MT Job, and that one should seek for a place in the arrangement of 4Q99 fragments where frag. 23 would fit given its other letters and the *vacat* in its second line. This approach is successful for lines 1 and 2: supposing that עֲנִיִּים is a textual variant, it is likely that the fragment should be placed at the left of frags. 7 ii and 15, and to the right of

ZAW 106 (1994): 307–22, esp. 307–8, who includes in his listing of texts covered by 4Q99: “34,28 f.(?).” Note, however, that Nebe includes other references, such as 7:11–13 which do not correspond with the eventually published identifications.

frag. 16, in lines 11–13 of the cluster of fragments published as 4Q99 frags. 7 ii, 12–16 col. i, resulting in the following reading:

אל תשאף הלילה ל[על]ות עניים ת[ח]תם 11
 [] vacat [אל א]ן [השמ]ר אל תפן [אל א]ן 12
 [] כי ע[ל] זה בחרת מ[עני] הן [א]ל ל[בדו]? 13

In line 11, 4Q99 frag. 23 line 1 provides the word עניים where MT Job 36:20b has עמים, a variant which may go back to the possible confusion of *mem* with the “ligature” *nun-waw* or *nun-yod*. (6) In line 12, the last two traces of frag. 23 line 2 match the reconstructed reading of Job 36:21a. Similarly, the uninscribed part of the skin matches the *vacat* we know should be there, given that Job 36:21b starts in the next line. Difficulties with this material-textual approach arise only in line 13. Very few and minute traces remain on frag. 15 after the *mem* of מ[עני], and the editors’ reading [א]ל[נ]ה is difficult. (7) On the basis of MT, however, one cannot explain the two strokes which look like the masts of two adjacent *lamed*s. The first of those would perfectly fit the mast of the *lamed* of אל. The second might derive from the following word, (8) but MT has here ישגיב, so that we would have to assume a variant or scribal error, neither of which is unlikely, given the fair amount of variants in 4Q99. If we have a variant (rather than an error), the letter *lamed* might plausibly be associated with the reading of the medieval Job targum, which reads here בלחודדי אלהא תקף בחלילה, having here, vis-à-vis the MT and the other ancient versions, the plus בלחודדי, “he

(6) Cf. Raphael Weiss, “On Ligatures in the Hebrew Bible (נ = ם),” *JBL* 82 (1963): 188–94. Weiss discusses the confusion of *nun-waw* with final *nun*, but given the similarity of *waw* and *yod* in many Dead Sea manuscripts, this also goes graphically for *nun-yod*. The mistake would happen most easily with final *mem*, which also occasionally is written in non-final position, but could also, though less easily, occur with medial *mem* and particularly *nun-yod* (rather than *nun-waw*).

(7) See the description in DJD XVI, 176. Puech, “Glanures épigraphiques,” 286, differs minimally, reading *h|nh* *’|l*. It is uncertain that we have here a horizontal base-stroke suggesting *nun*. The horizontal trace is rather short, and I propose it may be the foot of *’alep* turning slightly rightward, even though none of the preserved *’aleps* in 4Q99 has an extended rightward foot. The two downstrokes are too close to each other for *he*, and out of context one could read here *’alep* followed by a letter with a downstroke. Here, we may perhaps read אל, in the way the combination *’alep-lamed* is written in 4Q99 frag. 4 line 3, where the hook of *lamed* is written close to the left leg of *’alep*. Nonetheless, the reading אל fits possibly, but not perfectly, with the preserved traces.

(8) The first mast of *lamed* suggests a mast leaning strongly to the left, as occasionally with word-final *lamed*, such as, most extremely, in 4Q99 4–5 line 7 כל and 7 ii, 12–16 i line 11 אל. The following, word-initial *lamed* has a straight vertical mast.

alone,” which elsewhere renders Hebrew לבדו. The possibility of the reading לבדו is strengthened by the collocation of לבדו and the verb שגב in Isa 2:11 and 17 ונשגב יהוה לבדו (9). Graphically, however, a reading הן אל לבדו ישגיב בכחו is too long for the remaining part of the line, where הן אל לבדו ישגיב/נשגב might just fit.

In spite of the necessity to assume two variants, in line 11 (עניי) and in line 13 a word starting with ל (e.g., לבדו), the fragment fits textually and materially in this location of 4Q99.

4. Explaining the Variants

Job 36:19–20 is notoriously difficult, to the extent “that many critics omit them in despair” (10) and do not translate them. (11) Those who do translate 36:20, render something like “Do not long for the night, when peoples are cut off in their place” (NRSV), or offer translations based on emendations and/or other understandings of the terms, such as “N’écrase pas ceux qui te sont étrangers, pour mettre à leur place ta parenté” (Bible de Jérusalem).

Job 36:19–20 has not been fully preserved in 4Q99, but the extant text corresponds to the MT, with the exception of the reading עניי/עניי in 36:20 for MT עמי. One can hypothesize that the scribe had a copy with the MT reading עמי, but did not understand it in the context, and misread the word for עניי, and subsequently corrected it to עניי. However, given the problems of understanding the text, one should not privilege the reading of the MT, and also consider the reading of 4Q99, עניי, in any search for understanding the text. At the least, this manuscript provides a new variant in this difficult verse.

The second variant (in Job 36:22) is only witnessed by one letter, a *lamed*, in a hemistich where most scholars are content with the Hebrew. Suggestions for emendation have been limited, such as, tentatively, by Samuel Driver and George Gray, who suggest that ישגיב might be an error through dittography of (ישגי = ישגה) (12) and do not correspond

(9) Thanks are due to Rebekka Luther for suggesting the reading ללבדו and the references to the Job targum and to Isa 2:11 and 17.

(10) Marvin H. Pope, *Job: A New Translation with Introduction and Commentary*, AB 15 (New York: Doubleday, 1973), 270.

(11) Among many others, Artur Weiser, *Das Buch Hiob übersetzt und erklärt*, ATD 13; 2nd ed. (Göttingen: Vandenhoeck & Ruprecht, 1956), 232, does not translate the text. Franz Hesse, *Hiob*, ZB (Zurich: TVZ, 1992), 187 n. 221 “Die Versen 19 und 20 sind überhaupt nicht zu übersetzen.”

(12) Samuel R. Driver and George B. Gray, *The Book of Job*, ICC (Edinburgh: T&T Clark, 1921), part 2, 281.

LE TARGUM DE JOB DE LA GROTTTE 4 :
4Q157 = 4QTGJOB

Des deux petits fragments attribués à ce rouleau, un seul avec des restes de deux colonnes a été identifié comme appartenant aux premiers chapitres du livre, Jb 3,5–9 et 4,16–5,4. (1) Le mérite revient tout d’abord à son éditeur, J.T. Milik, d’avoir identifié le fragment principal. En fait le déchiffrement du frg. 2 non identifié dans l’édition vient se placer en bas de 1 ii 10–11. L’écriture est du type hérodien tardif, dans le deuxième quart du 1^{er} siècle de notre ère, comparable à la copie de 11Q10. Un réexamen des photographies (PAM 41.945, 42.198 et 43.449, ainsi que des photographies numérisées de la Leon Levi Dead Sea Scrolls Digital Library B-358468/9 et B-358470/1) permet quelques précisions de lecture. L’écriture se situe régulièrement au-dessous des lignes bien visibles, intercolonnement de 1,4 cm et interlignes entre 0,45 et 0,5 cm, hauteur moyenne des lettres de 0,2 cm.

1 - 4Q157 fragment 1 i = Jb 3,4–8

וְלֹא	4	1
תִּשְׂרָא עֲלוֹהִי עֲנָנָא	5	2
בִּירְמִי שְׁנָה	6	3
לִילִיא הַהוּא יְהֵא	7	4
תִּחְשְׁכוּ כֹכְבֵי מַמְ	8	5

(1) Voir J.T. Milik, « V Targum de Job 157 », in *Qumrân Grotte 4 II. - I. Archéologie* par R. de Vaux, II. *Tefillin, Mezuzot et Targums (4Q128–4Q157)* par J.T. Milik, DJD VI (Oxford : Clarendon Press, 1977), p. 90. K. Beyer, *Die aramäischen Texte vom Toten Meer* (Göttingen : Vandenhoeck & Ruprecht, 1984), ne s’occupe que du frg. 1, de même *The Dead Sea Scrolls Study Edition. Volume One 1Q1–4Q273*, ed. by F. García Martínez & E.J.C. Tigchelaar (Leiden - New York - Köln : Brill, 1997), 302–305.

Notes de lecture :

- Ligne 1 : Au-dessus des deux *nun*, ligne 2, trace de trait fin tiré à gauche touchant presque l'axe du *alef*, pour *lamed* ou *mem*, puis extrémités typiques de l'axe de *alef*
- Ligne 4 : Sur la photographie B-358468, reste du haut de la partie centrale d'un *alef*, puis traces de *yod* et plus clairement de *he* et de *alef*, lecture assurée. (2)
- Ligne 5 : Lecture certaine de *het*.

Traduction

1		4	<i>et que] ne[]</i>
2	<i>[brille] pas[sur lui l'aurore</i>	5	<i>que s'installe sur]lui le nuage</i>
3	<i>[</i>	6	<i>aux jou]rs d'une année</i>
4	<i>[</i>	7	<i>cette nuit-l]à soit</i>
5	<i>[</i>	(8)	<i>⁹Que s'obscurcissent les étoiles du crépus]cule,</i>

Commentaire

- Ligne 1 : Les restes peuvent être lus *]m'[]* ou *]l'[]*, comme *yw]m'[]* est en début du verset 4 absent de quelques manuscrits grecs (B ++), il paraît plus recommandé de lire *]l'[]* qui peut appartenir à plusieurs possibilités : *qb]l'[]*, *]l'[]*, *mn l']l'[]*, et *w]l'[]*, le début du verset 3,4b.
- Ligne 2 : Les restes traduisent la finale de 3,5a, mais le substantif araméen est à l'état emphatique contrairement à l'hébreu sans article.
- Ligne 3 : Restes de la fin de 3,6a.
- Ligne 4 : La lecture assurée de *hhw]'] yh'* rattache ces restes à 3,7a de l'hébreu pour restaurer vraisemblablement le mot *lyly'*.
- Ligne 5 : La seule lettre conservée est sujette à restauration. Milik a proposé de restaurer *mdn]h* comme traduction de l'hébreu *nšpw* qu'il compare à *nwgh' ddnh* du syriaque, pour 3,9aa. (3) Beyer propose *mm]h* « du crépuscule ». (4) Toutefois, le souhait du v. 9 que s'obscurcissent les étoiles s'accommode mieux du crépuscule où les étoiles commencent à briller avec la venue de la nuit, ainsi le grec B, que de l'aube où les étoiles pâlisent d'elles-mêmes avec la venue de la lumière.

(2) Milik, *cit.*, p. 90, ne lit pas ces restes. Beyer, *op. cit.*, p. 284, propose de lire et de restaurer *hd]wh*, mais lecture impossible avec le *alef* qui suit. García Martínez & Tigchelaar, *op. cit.*, p. 302, ne lisent que les restes des lignes 2 et 3.

(3) Milik, *cit.*, p. 90. P. Dhorme, *Le livre de Job*, Études Bibliques (Paris : Gabalda, 1926), p. 26, rappelle que *nšp* en hébreu peut signifier « crépuscule et aube », et il retient le sens de « aube » comme le syriaque.

(4) Beyer, *op. cit.*, p. 284, propose *mm]h* « du crépuscule ». A *Hebrew and English Lexicon of the Old Testament*, by F. Brown, S.R. Driver and A.A. Briggs (Oxford : Clarendon Press, 1968), p. 676a, classe Jb 3,9 sous « evening twilight », ce qui paraît plus logique dans ce contexte.

2 - 4Q157 fragments 1 ii + 2 = Jb 4,16–5,6

[ולא	16			0
[אכ[יר חוזה	1
[18		האנש מן] אלהא יצדק	2
[19		ובמלאכ[הי ישים	3
[20	ובעפרא]	4
[ומן בלי מנין שנן לעלמא יהובדן ²¹ הלא	5
[ימותן ולא בחכמתא ¹ [קרי נ]א ולמן מן קדישיא	6
[תבקה ² הלא סכל יק[טל רוגא ו.... תמית קנאתא	7
[ואנה חזית דדמע בעה ולטת לא[תרה בתכיף ⁴ ירחקק בנזחי מן]	8
[פון]רקן והתקדישו בי תרע ⁵ vac. די חצדין כפנין יאכלון]	9
[ו]ל[וינסכ[ון לצהין הון ⁶ ארו]	10
[לא נפק מן עפרא ומן אדמתא] לא יצמח עמל	11

Notes de lecture

– Ligne 1 : Après *alef*, le tracé peut convenir à *kaf* ou à *nun*, mais très difficilement à *bet*, à *šade* ou à *mem* (Beyer). Un point d'encre à l'extrémité de la base ne peut appartenir à un jambage vu la tête du *kaf*. Une lecture *'n[kyr* (proposition(?)) de Milik, suivi par García Martínez & Tigchelaar) est à exclure pour une séquence *nun-kaf*.

– Ligne 2 : Contrairement à l'édition et à García Martínez & Tigchelaar, on doit clairement lire *mn* avant la cassure.

– Ligne 3 : Le premier *waw* a un tracé de *yod*, puis jambage de *waw* à la cassure.

– Ligne 4 : La trace d'encre devrait être celle de la crosse de *waw* au jambage disparu dans les écailles à la marge de droite. Milik propose *dalet*(?) sans doute influencé par l'hébreu et le syriaque, de même García Martínez & Tigchelaar, mais sans traces de cette large lettre dans la marge.

– Ligne 5 : Le *yod* est de lecture assurée.

– Ligne 6 : Des traces d'encre assurent la lecture *bet-ḥet-kaf-mem-taw-alef*, *bḥkmt'*. Puis une trace d'encre au-dessus du *kaf*, ligne 7, peut convenir au *nun*.

– Ligne 7 : Le premier *taw* est partiellement lisible. À la cassure, crosse de *qof*.

– Ligne 8 : La lecture de cette ligne a fait difficulté. Milik a lu en pointant les lettres *drš' m[w] 'h* et Beyer *ršy'* [sans pointer les lettres. (5) Les photographies B-358468/9 demandent de lire *ddm' b'h*, (*mem* puis *bet* seuls possibles), et à la cassure vraisemblablement le jambage droit de *alef* avec *apex*.

– Ligne 9 : De maigres restes de la tête de *reš*, puis *qof-nun* final, un espace blanc, *waw-he-taw-qof-dalet*, probable tête de *yod*, puis *šin-waw*, puis *bet-yod*, puis *taw-reš- 'aīn*, et *vacat*. (6)

(5) Beyer, *op. cit.*, p. 284, et a traduit «Und Ich sah einen Frevler [». García Martínez & Tigchelaar, *op. cit.*, p. 304, suivent Milik pour *drš'*.

(6) Cette séquence n'a pas été lue par l'éditeur ni par Beyer ni par García Martínez & Tigchelaar, *op. cit.*, p. 304.

– Ligne 10 : Hampe de *lamed* sous le *taw*, et sur le frg. 2 1 qui est à placer dans cette ligne, (7) après un *vacat*, lire *waw-yod* (difficilement *he*), puis *nun*, partie de *samek* et coude du *kaf*.

– Ligne 11 : Au frg. 2 2, la lecture]'dmt'[est certaine.

Traduction de Jb 4,16–5,6 araméen :

[¹⁶ ¹¹je [/ne] recon[nus pas sa figure ...]
² ¹⁷Est-il un homme plus [juste] que [Dieu ... ? ¹⁸...]
³ et à [ses] anges[Il impute ... ¹⁹...]
⁴ et dans la poussière [... ²⁰...]
⁵ et sans nomb[re d'années(?) ils périssent à jamais ²¹...]
⁶ ils meurent, et privés de la sagesse ? [^{5.1}Appelle-]do[nc et vers lequel des saints]
⁷ te tourneras-tu ? *vacat* ²N'est-ce pas un insensé que tu[e la colère, et un sot que fait mourir la jalousie ?]
⁸ ³Et moi, j'ai vu que criait un suppliant et j'ai maudit [sa] p[lace aussitôt(?).
⁴Ses fils seront sans]
⁹ salut *vacat* et ils ont été condamnés à une(/la) porte ! *vacat* [⁵Ce que les affamés ont récolté, ils mangeront]
¹⁰ [et les]:[] *vacat* et [on] versera [aux assoiffés(?) leur ⁶Parce que]
¹¹ [ne sort pas du sol et de]la terre[ne germe pas la peine.

Commentaire

– Ligne 1 : Les restes conservés traduisent l'hébreu *wl' 'kyr mr'hw*, le verbe est bien attesté en araméen, ici au *'af'el*. La négation *wl'* devait être lue à la ligne précédente.

– Ligne 2 : Le verset 17 commence par une interrogation *h-* comme l'hébreu, puis l'araméen *mn* lit une comparaison entre l'homme et Dieu au sujet de la justice comme le syriaque contrairement au grec ἐναντίον. Comparer *m' yšdq* en *11Q10* 25,4 à restaurer ensuite.

– Ligne 3 : La ligne commence avec 4,18b « et à [ses] anges » comme en hébreu, en grec et en syriaque, mais différent du targum de Jonathan.

– Ligne 4 : La traduction araméenne a changé(?) la structure du passage en lisant la coordination et non le relatif devant le complément.

– Ligne 5 : Milik propose de restaurer *wmn bly mny[h*, « et sans celui qui procure du repos » pour traduire l'hébreu *mbly mśym* qui fait difficulté. Beyer propose de restaurer *wmn bly mny[n* «und ohne Zahl[?]. Dans un contexte d'une vie brève accordée à l'homme, poussière, qui du matin au soir est broyé, on comprendrait assez bien une restauration

(7) Le fragment 2 est non identifié dans l'édition, Milik, *cit.*, p. 90, par Beyer, *op. cit.*, p. 284.

wmn bly mny[n šnyn l'lm yhwbdwn « et sans un nomb[re d'années, ils périssent à jamais ». (8) Le manuscrit est le seul à user de la coordination à *wmn bly*, alors que le TM et les autres versions ont une juxtaposition.

– Ligne 6 : Le début de la ligne traduit verbalement le v. 4,21b hébreu. Le verset devait commencer par l'interrogation négative *hl'*... à la ligne précédente. La trace d'encre pourrait être celle du *nun* médian de *[n]*' pour lire ainsi le début du v. 5,1 : *qry [n]' h'yty 'nh lk* « Appelle [do]nc, y a-t-il quelqu'un qui te réponde ? »

– Ligne 7 : Le verbe *bqh* en *11QtgJb* 36,25 traduit l'hébreu *nbṭ*, et en *4QtgJb* 5,1 il traduit l'hébreu *pnh*, pour signifier « tourner son regard ». Après un *vacat* le verset 2 commence par une interrogation négative, dont le sujet est traduit en grec par ὅπῃ que l'araméen rend par *rwgz'*, ainsi aussi le syriaque et le targum. Un hémistichie parallèle complète le verset.

– Ligne 8 : Lire au mieux *w'nh ḥzyt ddm' b'h wltt l'[trh* de 5,3. (9) Le verset 3 commence par une coordination absente du TM et du targum, mais présente en syriaque, voir δὲ du grec qui lit un pluriel. Excepté les deux premiers mots, la suite est sans parallèle, seul le début du deuxième hémistichie suit le TM ainsi que les versions, en restaurant sans doute *btkyp* « aussitôt » à la fin.

– Ligne 9 : La distance à la marge ne permet pas de restaurer deux grandes lettres telles que *[mp]rqn* d'une part (10) et, d'autre part, le manuscrit détache la préposition *mn* du substantif (voir ligne 2) autorisant de restaurer uniquement *[pw]rqn* et de reporter la préposition à la fin de la ligne 8, même orthographe qu'en *11Q10* frg. 14 5 à lire *pwr]qny* = Jb 30,15. L'araméen devrait suivre le TM en 5,4a en restaurant *yrḥqwn bnwhy mn*] à la ligne 8, verbe à l'inaccompli, puisque en 4b le verbe est au parfait *hetpe 'el*, non à l'inaccompli comme en hébreu, en syriaque et dans le targum. (11) La suite est de lecture assurée, mais le sens de 5,4b diffère du TM. La lecture *htqdyšw* paraît la seule possible au sens de « être mis à part, être condamné ». Le traducteur a compris une mise à part/une condamnation à la porte (de la ville) où est rendue

(8) Voir dans le même sens le targum *mn dlyt mšwy lhwn 'rk'*. Beyer, *op. cit.*, p. 284, lit ainsi et traduit «] und ohne Zahl [», lecture retenue par García Martínez & Tigchelaar, *op. cit.*, p. 305.

(9) D. Shepherd, « What's in a Name? Targum and Taxonomy in Cave 4 at Qumran », *Journal for the study of the pseudepigrapha* 17 (2008) 189–206, p. 193, accepte la lecture de la ligne de l'édition, et en tire une conclusion pour les différentes traductions de l'hébreu *'wyl* par *ršy'* mais le texte du manuscrit est différent. R.I. Vasholz, « 4QTargum Job versus 11QTargum Job », *RdQ* 41 (1982) p. 109, soulignant les différences orthographiques entre les deux targums qumraniens, seule est à retenir le relatif *d-* à la ligne 8.

(10) Ainsi Milik, *cit.*, p. 90 et Beyer, *op. cit.*, p. 284.

(11) Malgré Shepherd, *cit.*, p. 191–192.

la justice : en conséquence, il n'a pas traduit les deux derniers mots « et sans défenseur » présents dans le TM et les autres versions, qu'il a dû juger inutiles après une condamnation comme le suggère le *vacat*. L'expression *by tr'* est pour *btr'*, « à une porte », littéralement 'une maison de porte'.

– Ligne 10 : Le *lamed* peut appartenir soit au verbe *y'k*][*wn*, à un autre verbe *y*][*qḥwn* ou à la négation '][(voir le grec) ou à une préposition. La compréhension reste difficile. Après un *vacat*, le verbe *wynks*[*wn* devrait commencer le v. 5,5b à compléter peut-être par *lshyn* (voir 11Q10 - Jb 22,7), le verset étant au pluriel contrairement au TM.

– Ligne 11 : Est préservé le début du deuxième hémistich de v. 5,6b, faisant pendant à *mn 'pr'* de 5,6a. L'hébreu '*wn* du TM en 5,6a a compris « la peine/la fatigue » de même le grec κόπος ὀδύνη (B/Sym), mais le syriaque et le targum ont traduit « le mensonge », il est impossible de savoir comment a lu le traducteur de 4Q157. Avec *wmn 'dmt'* en 5,6b le manuscrit appuie *wm'dmh* du TM, du syriaque et du targum, non le grec ἐξ ὀρέων ou le latin *de montibus*.

Ainsi lus malgré des passages mal conservés et non assurés, les maigres restes de cette copie ne sont pas sans intérêt comme témoins du texte de ces passages de *Job*. La traduction araméenne semble rendre compte de quelques variantes soit de la *Vorlage* hébraïque ou de compréhensions divergentes de l'hébreu. Il est aussi à noter qu'une version du targum rabbinique partage plusieurs mots avec ce manuscrit. (12) Mais les restes de 4Q157 en font une traduction araméenne assez littérale, ainsi qu'il apparaît dans ces quelques versets de Jb 3 à 5, plus que celle du targum rabbinique plus tardif avec des expansions et des interpolations sur les vues eschatologiques du monde à venir et du grand jugement, *e.g.* en 5,4. (13) En cela il rejoint plusieurs fois la traduction grecque et même syriaque, même s'il est impossible de connaître le texte hébreu de base qui pouvait être aussi quelque peu différent du TM reçu. Il est encore à relever les nombreux *vacat* dans cette petite portion de texte préservé, des blancs qui ne correspondent pas aux coupures massorétiques et qui supposent une autre structure des hémistiches. (14)

(12) Plusieurs des mots préservés se retrouvent dans un manuscrit du targum de Job, voir L. Díez Merino, *Targum de Job. Edición Príncipe del Ms. Villa-Amil n. 5 de Alfonso de Zamora*, Biblia Poliglota Complutense. Tradición sefardí de la Biblia Aramea IV,2 (Madrid : Consejo Superior de Investigaciones Científicas Instituto «Francisco Suárez», 1984), p. 128–130, mais moins nombreux dans le targum de Jonathan Ben Uzziel.

(13) Shepherd, *cit.*, p. 203, est d'un avis différent, et hésite même à qualifier cette traduction de targum. On peut ne pas le suivre.

(14) E. Kutsch, «Die Textgliederung im hebräischen Ijobbuch sowie in 4QTgJob und in 11QTgJob», *BZ Neue Folge* 27 (1983) 221–228, p. 228, relève les *vacat* entre

Enfin, il est évident que ces fragments du 1^{er} siècle ne sont qu'une copie du targum et non l'original, et ils ne présagent en rien de la date de la traduction araméenne, qui peut être bien plus ancienne, comme c'est le cas de 11Q10 - 11QTargum Job. (15) Lui était-elle contemporaine ?

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les versets 5,1–2 et 5,4–5, mais on ne peut pas en déduire que la copie opère régulièrement ainsi entre chaque "verset", voir le *vacat* en 5,5.

(15) Voir E. Puech, « Le livre de Job à Qumrân », in Angelo Passaro - Giuseppe Bellia (edd.), *The Book of Job. Studies in Tradition, Redaction, and Theology* (Trapani : Il Pozzo di Giobbe, 2020) (à paraître).

FILLING GAPS IN 4QNARRATIVE AND POETIC COMPOSITION: ANOTHER FRAGMENT OF 4Q373A

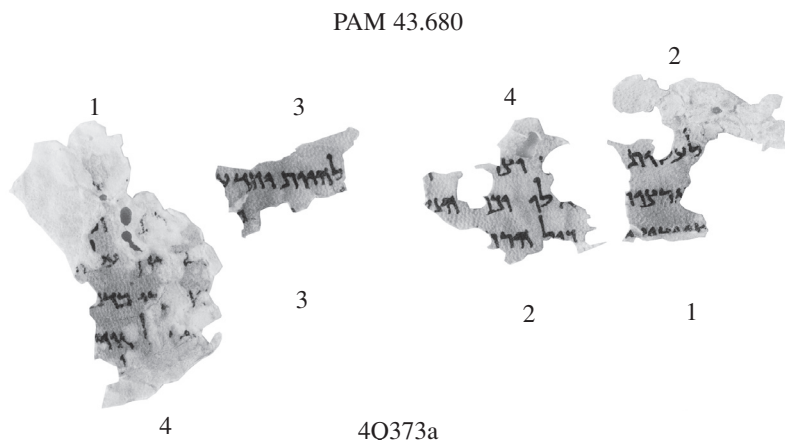
IN 2004 I discovered that the textual remains of the hitherto unidentified fragments PAM 43.680 frags. 1–3 corresponded to several lines of the work published as 4QNarrative and Poetic Composition, which is attested in 2Q22, 4Q371, 4Q372, and 4Q373. (1) Since these PAM 43.680 fragments do not belong to any of those manuscripts, they would derive from a fifth manuscript of the work, to be referred to as 4Q373a (4QNarrative and Poetic Composition^d). (2) Virtually all the words on these 4Q373a fragments were already known from the text of 4Q372 1 9–14 and 4Q371 1a-b 7–13. Unfortunately, the reading of PAM 43.680 frag. 1 line 3, which would help restoring the text of the end of 4Q372 1 11, was barely legible apart from the last word. I tentatively restored וּנְבִלִים חוֹשְׁבִים קֶלֶן עֲלֵיהֶם, “and fools were contemplating disgrace against them,” but Elisha Qimron offered an easier reading

(1) Eibert Tigchelaar, “On the Unidentified Fragments of DJD XXXIII and PAM 43.680: A New Manuscript of 4QNarrative and Poetic Composition, and Fragments of 4Q13, 4Q269, 4Q525 and 4QSb (?),” *RevQ* 21/83 (2004): 477–85, esp. 481–83. The manuscripts 4Q371–4Q373 (4QNarrative and Poetic Composition^{a-c}) have been published by Eileen Schuller and Moshe Bernstein in *Qumran Cave 4: XXVIII Miscellanea, Part 2*, ed. Bernstein et al., Discoveries in the Judaean Desert 28 (Oxford: Clarendon, 2001), 151–204. For 2Q22, then still named “Un apocryphe de David (?),” cf. the edition by Maurice Baillet in *Les ‘petites grottes’ de Qumrân: Exploration de la falaise; Les grottes 2Q, 3Q, 5Q, 6Q, 7Q à 10Q; Le rouleau de cuivre*, Discoveries in the Judaean Desert of Jordan 3 (Oxford: Clarendon, 1962), 81–82. For a composite edition of the five manuscripts, with new readings and the identification of the overlap between 4Q372 frag. 4 and 4Q373 frag. 1, cf. Elisha Qimron, *The Dead Sea Scrolls: The Hebrew Writings*, Vol. 2 (Jerusalem: Yad Ben-Zvi, 2013), 77–83.

(2) As included in Emanuel Tov, *Revised Lists of the Texts from the Judaean Desert* (Leiden: Brill, 2010), 47, though with the addition: “(temp. name).”

ונבלים יושבים עליהם, “and fools were dwelling on them (sc. the mountains).” (3)

Neither I, nor Qimron, nor other studies, considered the possibility that PAM 43.680 frag. 4 also derives from the same manuscript. The high quality photograph of PAM 43.680 on the IAA website shows that the surface of the fragment is identical to that of frags. 1–3. (4) The hand is plausibly the same, even though there are not enough letter samples to compare the script of the fragments with full confidence. It was most presumably on the basis of the similarity of hand and skin that these fragments were placed together, all four on PAM 43.680, and frags. 2–4 already on PAM 43.163 (upper left). The association of the fragments is also textually supported, since frag. 4 fits in the same lines as frags. 1, 2, and 3.



The transcription of PAM 43.680 frag. 4 (4Q373a frag. 2) that was given by the editors of the fragment (5) should be somewhat revised, and I read the following:

]◦ []◦ וצ◦ [1
]א לו וכו[ל] הנש]	2
]כול הרר]	3

(3) Qimron, *The Hebrew Writings*, Vol. 2, 78. This paper follows Qimron’s reading because it is textually more straightforward, not because it makes a better sense of the traces on PAM 43.680 frag. 1 line 3.

(4) The new IAA color photograph shows that its color is identical to frag. 1, while frag. 3 has turned much darker.

(5) Dana M. Pike and Andrew C. Skinner, *Qumran Cave 4, XXIII: Unidentified Fragments*, Discoveries in the Judaean Desert 33 (Oxford: Clarendon, 2001), 148–49.

If this fragment is placed to the left of PAM 43.680 frag. 2 (4Q373a frag. 1), then there is a possible overlap of line 3 כול הרר[יהם] with the text of 4Q372 1 11 כל הריהם, while the text of line 2 supplies part of the end of 4Q372 1 i 10. On the basis of this textual identification with 4Q372 1 9–11 one may also propose readings for the uncertain letters of line 1 of the fragment, and tentatively restore some of the gaps. This then could result in the following transcription of 4Q373a 1–4 lines 1–3: (6)

1 לעשות] משפ[ט וצדק]ה ו[] להיות יחד ע[ם שני אחיו ובכל זה יוסף מוטל]
2 [ב]ארצות[] לו וכול[] הנש[]בם [ל]קה[]ם ב[] גיר נאכר [זב]כול תבל[]
3 מִפִּזְפִּזִּים [vac] כול הרר[יהם] שממים מהם ונבלים יו[ש]ב[ם] על[]ה[ם]

to do justice and righteousness, and [] to be together with his two brothers. And in all this, Joseph was hurled into lands that were not his, and all the captives were taken among a foreign nation, and dispersed in all the world. All their mountains were desolate of them, and fools were dwelling on them

The reading of the first line is the most uncertain one, since only *waw-šade* is certain, and a reconstruction on the basis of these two letters would seem audacious. However, on the basis of 4Q372 1 23 which reads *וצדקה משפט לעשות ואקום* (“And I will arise to do justice and righteousness”), one may tentatively suggest the same phrase *לעשות משפט וצדקה* here, (7) especially since the other traces are compatible with this reading. (8)

According to my placement of PAM 43.680 4, (9) line 2 explains that Joseph was hurled into lands “which are/were not his,” i.e., “foreign” (לוא לו), rather than into lands “which he did not know” (לא ידע). (10) The latter was suggested by the edition of 4Q372 1 10, probably on

(6) From right to left, PAM 43.680 frags. 2, 4, 3, and 1, also to be referred to as 4Q373a frags. 1, 2, 3, and 4.

(7) For the phrase *משפט וצדקה* or *משפט וצדק*, see also 4Q372 18 3. This phrase, which is relatively common in the Hebrew Bible (26×), is very rare in the Dead Sea Scrolls (apart from 4Q372 only in 11Q5 26:11).

(8) Left top and part of the base of *ṭet*; downstroke of *dalet*. The new IAA colour photograph of Plate 71 shows that on the place where one would expect traces of *qop* (of *וצדקה*), the upper layer of the skin has been lost.

(9) The main challenge is the exact arrangement of the four fragments, and mapping the text of 4Q371 and 4Q372 (and the reconstructions) on these four 4Q373a fragments. This could result in lines of somewhat uneven length. In the transcription given above, a tentative match is given of two of the traces of PAM 43.680 frag. 3 2, but the first trace of this line, if ink, has been ignored.

(10) The minimal remaining trace read as *yod*, could also fit part of the hook of *lamed*.

the basis of Jer 22:28 “cast away in a land that they do not know,” (11) and adopted by me and Qimron. With the reading **בארצות לוא לו**, the expression is intertextually related to Gen 15:13, “Then the LORD said to Abram, “Know this for certain, that your offspring shall be aliens in a land that is not theirs (**בארץ לא להם**), and shall be slaves there, and they shall be oppressed for four hundred years.” Here the singular **לו** is used, as a reference to “Joseph,” even though he symbolizes the exiled northern tribes. And, whereas Gen 15:13 relates specifically to the land of Egypt, here a plural is used to refer to the different lands of exile of the northern tribes.

The words following **לו** in line 2 fill partially a gap in the other manuscripts, which Qimron had restored to **וישראל מפרפרים**, (12) reconstructing **מפרפרים** as parallel to the following **מפ(ו)צפצים** because **פרפר** and **צפץ** are used together in Job 16:12. However, rather than “Israel,” the fragment has here **כ[ו]ל הנש[ו]ל**, and the following words should probably match the scant traces of PAM 43.680 frag. 3 line 2. A word beginning with **הנש[ו]ל** can be restored in different ways. For example, since CD 14:15 refers to be taken captive (*nifal* שבה) by a foreign nation, one might tentatively restore here a *nifal* participle **הנשבים**, “the captives” (but other texts, such as 4Q385a 18 i 3, prefer a *qal* passive participle **שבוים** or **שבאים**). A more common and idiomatic restoration would be **הנשארים**, typically referring to the remnant after exile. The word should be followed by one with a *lamed* at or close to the beginning, and the tentative reconstruction **ל[קוח]ים** (or **ל[קוח]ים**) aims to restore a participle. For the passive **לקח**, “to be taken (into captivity),” see Jer 48:46.

The remaining text of line 3 overlaps with the text of 4Q372 1 11, be it with the morphological variant **הררי** rather than **הרי** with suffix (for which cf. also Deut 8:9).

Since 4Q372 preserves the most extensive text of these lines, it is helpful to place the text of the 4Q373a fragments, including PAM 43.680 frag. 4, in a transcription of 4Q372 1 lines 9–11 (13) (with PAM 43.680 1–3 single underlined and PAM 43.680 4 double underlined):

9 אַל וגם יהודה יחד עמו והוא על אם הדרכים יעמוד לע[שות משפט וצדקה ו]
10 להיות יחד עם שני אחיו ובכל זה יוסף מוטל בארצות לא לו וכל הנשבים לקוחים
11 בגוי נאכר ובכל תבל מפצפצים כל הריהם שממים מהם ו[גבילים ישיבים עליהם]

(11) Cf. also Jer 16:13 “Therefore I will hurl you out of this land into a land that neither you nor your ancestors have known” (**על הארץ אשר לא ידעתם**). In both Jer 22:28 and 16:13 the verb **טל**, “hurl,” is used similarly to our work.

(12) Qimron, *Hebrew Writings*, Vol. 2, 78, reading **בארצות לא לו**, “And in all this, Joseph was hurled into lands he did not k[now], and Israel was shattered] among a foreign nation, and dispersed in all the world.”

(13) Based on Qimron, 78.

This presentation of the lines of 4Q372 with the evidence of 4Q373a shows clearly the main contribution of PAM 43.680 frag. 4, namely that it helps to fill part of the gaps at the end of 4Q372 1 9 and 10. (14)

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(14) Publication of the figure of the four fragments: Courtesy of the Leon Levy Dead Sea Scrolls Digital Library: Israel Antiquities Authority. I wish to thank Eileen Schuller for her comments on an earlier draft of this paper. The author is also a Research Associate of the University of Pretoria.

RECENSIONS

Jan Joosten, Daniel Machiela, Jean-Sébastien Rey (eds.), *The Reconfiguration of Hebrew in the Hellenistic Period: Proceedings of the Seventh International Symposium on the Hebrew of the Dead Sea Scrolls and Ben Sira at Strasbourg University, June 2014*, STDJ 124 (Leiden: Brill, 2018). Pp. vi + 282. ISBN 978-90-04-36677-0. € 110.00 / \$ 132.00.

It is quite evident that Qumran Hebrew deserves its very own place as a sub-discipline within the wider field of (Biblical) Hebrew study. The inertia that this sub-field has picked up in recent years has, at least in part, been due to the International Symposia on the Hebrew of the Dead Sea Scrolls and Ben Sira, which, since 1995, have served as a forum for ongoing discussion about the linguistic features of the Hebrew texts found at Qumran. This particular volume constitutes the proceedings of the Seventh International Symposium held in Strasbourg from June 22–25, 2014.

Interest in the multiplicity of Hebrew traditions of the Middle Ages and those from antiquity has been growing in recent years. Indeed, scholarship is beginning to distance itself from the (perhaps only subconscious) view that sees *Tiberian* Hebrew and *Biblical* Hebrew as interchangeable terms. The four-year gap between the 2014 symposium and its publication was even witness to the *Handbook of Biblical Hebrew* (R. Garr and S. Fassberg [eds.], Winona Lake, IN: Eisenbrauns, 2016), a two-volume work which brings together a wide array of Biblical Hebrew traditions side-by-side. All of this is to say that, while it may have been possible in the past, scholars of Biblical Hebrew can no longer afford to ignore the multiplicity and diversity of traditions from the medieval and ancient periods. It is no surprise, then, that the language tradition(s) of the Qumran scrolls should hold a monumental place among those of antiquity. The present volume is therefore a very welcome contribution in this ever-growing sub-discipline of (Biblical) Hebrew studies.

The book itself contains fourteen papers on a variety of topics, all of them linguistic in one way or another. Although a number of the papers argue persuasively for one particular linguistic point or another, a surprising number of them conclude by leaving the reader with a significant degree of uncertainty, or they do not include a concluding section at all (e.g., Muraoka). In the view

of the present reviewer, this is not due to any sort of scholarly laziness, but rather due to the carefulness of the authors' analysis. In fact, the authors repeatedly emphasize just how tenuous linguistic arguments can be when dealing with diverse and fragmentary sources like the Dead Sea Scrolls and Ben Sira. Though arranged in alphabetical order by author's name in the volume, the papers can be re-grouped into the following four categories: (i) lexicography, (ii) phonology/morphology, (iii) syntax, and (iv) diachrony and/or general linguistic profile.

To the first category (lexicography) belong the papers of Bar-Asher (ch. 1), Mizrahi (ch. 8), and Rendsburg (ch. 11). Bar-Asher weaves philological analysis with historical theology as he traces the reception of the phrase יהוה איש מלחמה 'the LORD is a man of war' (Exod. 15.3) from the Song of the Sea all the way to the Amoraic period, showing the hesitance of later generations to use איש as an appellation for God. Mizrahi unties the philological knot that has long encompassed the Qumran Hebrew phrase תכמי בשר 'body parts', finding in it an Aramaic loan specific to the register of apotropaic texts. Rendsburg re-examines the possible etymologies of the DSS Hebrew word זעטוט 'youth; young man' (Hebrew קטלול pattern, Aramaic זוטא, Greek ζήτητης), eventually favoring the native-Hebrew etymology.

To the second category (phonology/morphology) belong the papers of Fassberg (ch. 3), Reymond (ch. 13), and Yuditsky and Ariel (ch. 14). Fassberg reconsiders the typical explanation of the loss of gutturals and subsequent gemination in Samaritan Hebrew, crediting compensatory lengthening and quantitative metathesis, rather than guttural assimilation, for the secondary gemination. Reymond considers the possible attestations of a 3MS ה- (as opposed to ו-) suffix in the various manuscript witnesses of Ben Sira, identifying a small selection of cases in which Ben Sira appears to attest to this typologically archaic suffix. Yuditsky and Ariel survey the numerous apparent instances of ז ← ש in the Dead Sea Scrolls, arguing that such interchanges reflect an allophonic realization of *šade*—normally /s/ or /sʰ/—as a voiceless postalveolar affricate [č] (or [tʃ]).

To the third category (syntax) belong the papers of Eskhult (ch. 2), Holmstedt (ch. 4), and Novick (ch. 10). Eskhult traces the development of the *qatal* and *yiqtol* forms from Biblical Hebrew into Qumran Hebrew, arguing that the classical functions of *we-qatal* and *wayyiqtol* have mostly faded out of non-archaizing language. After calling attention to the lack of comprehensive descriptions of Qumran Hebrew syntax, Holmstedt suggests that we begin work on a descriptive grammar of each substantial text from the Qumran scrolls. Naturally, his current grammar on the War Scroll (1QM) serves as a prescriptive model for this endeavor. Novick surveys the various particles and words used in *a fortiori* argumentation, beginning with biblical אף כי 'all the more so' and ending with its eventual replacement in rabbinic קל וחומר 'all the more so' (literally: 'light/unimportant and heaviness/importance').

To the fourth, and largest, category (diachrony and/or the general linguistic profile of Qumran Hebrew) belong the papers of Hornkohl (ch. 5), Joosten (ch. 6), Machiela (ch. 7), Muraoka (ch. 9), and Rey (ch. 12). Hornkohl shows that, although there are many cases in which differences between Tiberian and Qumran Hebrew point to the relatively more conservative profile of the former,

there are also numerous examples in which Qumran is (at least on the surface) typologically more archaic, such as the 3MP verbal form יקטולו, the 3MS ending/suffix יהו-, and the gentilic suffix יים-. Joosten presents a number of pieces of data that can help establish a division between the linguistic profiles of Late Biblical Hebrew and Qumran Hebrew (e.g., pseudo-classicisms, religious vocabulary, use of the participle), which otherwise exhibit a high degree of overlap and even chronological proximity. Machiela re-assesses the linguistic features and profile of Hebrew Tobit, arguing along the way in favor of an Aramaic original being translated into Hebrew rather than the reverse. Muraoka surveys a select number of recurring linguistically-significant variants (orthographic, phonological, and morphological) in the book of Psalms at Qumran, such as the digraph וו for consonantal *waw*, ו- as the 3MS suffix on singular *and* plural nouns, the retention of the stem vowel in *yiqtol* verbs (e.g., תעוובי, תעוובי), and *binyan* alternation (e.g., יפרחו instead of יפריחו). Key points out a number of general problems with taking the Genizah manuscripts of Ben Sira as accurate reflectors of Hellenistic-period Hebrew, yet at the same time shows how a careful analysis of these sources can sometimes yield genuine linguistic data from antiquity.

All in all, this is a very fine volume and constitutes a significant contribution of many authors to the field of Qumran Hebrew studies. There are perhaps only two areas of lack—both are in connection with the title—that would have been nice to see included. First, the term “Hellenistic” is never explicitly defined. Are we to assume, as a classicist might, that the *terminus* of the period covered is the beginning of the Roman period in 37 BCE? Or, is the term “Hellenistic” used more generally to encompass the period in Palestine from the conquest of Alexander until the beginning of Byzantine control in 324 CE? Second, despite the intriguing title (*The Reconfiguration of Hebrew in the Hellenistic Period*), which might imply a substantial introductory or concluding chapter to flesh out the cumulative implications of the papers for shedding light on the transformation of Hebrew during the Hellenistic period, readers are left to connect the dots and paint a picture for themselves. Nevertheless, this is an excellent volume that deserves concerted attention from (Biblical) Hebrew scholars, especially those dealing specifically with Qumran and/or the Hellenistic–Roman periods.

Benjamin KANTOR

Mika S. Pajunen and Jeremy Penner (eds.), *Functions of Psalms and Prayers in the Late Second Temple Period*, Beihefte zur Zeitschrift für die alttestamentliche Wissenschaft 486 (Berlin/New York: de Gruyter, 2017). Pp. ix + 506. ISBN 978-3-11-044774-3. € 129.95 / US \$ 149.99.

The twenty articles of this volume are based on presentations delivered at two conferences, held in Copenhagen and Helsinki in 2015. As the title indicates, the aim is to illustrate the functions of psalms—the term “function” referring here to “the varied contexts in which psalms are employed and their

diverse purposes” (p. 2)—in ancient Israel and Jewish antiquity. The editors explicitly acknowledge multiple intentions and settings of psalms that include but are not limited to worship.

The editors’ concise introduction is followed by Eileen Schuller’s overview article on psalms, hymns, and prayers in late Second Temple Judaism. The essay presents the state-of-the-art in Qumran studies, introducing the reader to past developments and current conversations. The rest of the articles are divided into six thematic sections, each of which approaches ancient psalms and prayers from a specific angle.

Part 1 focuses on human experiences and embodied religion, including four studies that reach beyond form-critical inquiries. The articles analyse the act of blessing in 1QS in light of cognitive theory (Jokiranta), the effects of curses in Psalms of Solomon (Werline), notions of self in Jewish texts from the Second Temple era (Newsom), and ritual mourning in Daniel’s prayer (Harkins). Regarding the scrolls, Jokiranta explores how agency is constituted in the covenant ceremony, while Newsom shows their relevance for the development of the western introspective self.

Part 2 concentrates on penitential themes in three writings: Psalm 51, read in the context of the Axial Age thinking in the Persian era (Holt); 4QSapiential Work (4Q185), analysed with a focus on the themes of remembrance and repentance (Aadland); and the book of Lamentations, including its predecessors and afterlife (Körting). Based on her survey of a Qumran “wisdom text,” Aadland argues that we should look for traces of prayer beyond strictly liturgical texts, whereas Körting maps the long tradition of city laments from the ancient Near East to penitential prayer at Qumran.

Part 3 shifts attention to materiality and ordering of ancient liturgical texts. The articles cover scribal features in Psalms scrolls from Qumran (Davis), the material reconstruction of 4Q511 (Angel), and a warning against identifying subgroups of psalms within the MT Psalter based on the use of certain idiom and themes (Willgren). Davis aptly argues that the Qumran scrolls are more than “text containers.” Angel, in turn, addresses the question of sequence which is surely significant in textual analysis, but often inaccessible because of the fragmentary nature of the Qumran finds.

Part 4 contains two essays on psalms and prophecy. They discuss the evidence for psalms as prophetic texts in the Qumran scrolls (Høgenhaven) and two prophecy-related phenomena—the use of earlier psalms as prophecies and the composition of new psalms attesting to fresh prophetic expectations—in the Psalms of Solomon and the Barkhi Nafshi hymns (Pajunen). Both articles add to the debate on the role of prophecy in Second Temple Judaism, illuminating some of the ways in which the prophetic tradition not only persisted but was even vital at this period of time.

Part 5 highlights issues of history and identity. Attention is paid to the rhetorical function of post-exilic psalms in the construction of group identity (Brettler); the representation of Israel’s history in historical psalms and their afterlife in selected texts from Qumran (Brooke); and the invoking of family ties in historical psalms of the Hebrew Bible (Klein). Combining textual analysis with reflections on memory and liturgy, Brooke shows that the reuse of earlier

materials is more than an intellectual game: the appropriation may shape and mark identities, or point to performative acts.

Part 6 sheds light on the composition and use of psalms and prayers. The four articles analyse how the First Temple is imagined in Psalms 122 and 137, which originate from the Second Temple era (Berlin); how the author of Sirach draws on psalm genres as he keeps creating new poetry (Marttila); how Paul employs psalms and modifies them in Romans 3, in order to convince his audience (Pulkkinen); and how the view of Philippians 2:6–11 as a Christological psalm emerged in the 20th century scholarship (Justnes).

In spite of the selected focus on psalms and prayers, the collection of essays remains somewhat eclectic, as is typical of these kinds of volumes. As such, however, the book hopefully finds its way to the hands of many and prompts scholars to read beyond their own particular areas of expertise. It is especially delightful that many—though not all—of the articles bridge between Hebrew Bible studies and Qumran studies, a division that should not exist in the first place. Meanwhile, only a few of the articles examine Greek Jewish texts. All the writers are white and western, but it is encouraging that both sexes are equally represented.

Since this brief review has underlined the volume's relevance for the study of the Qumran scrolls, I should end with a note that it also raises many other issues worthy of attention. I found specifically attractive those essays that invite biblical scholars to leave their comfort zones and place ancient Hebrew and early Jewish writings in wider contexts of study, whether the question is about the Axial Age or the literary and cultural history of self-formation.

Elisa UUSIMÄKI

Katell Berthelot, *In Search of the Promised Land? The Hasmonean Dynasty between Biblical Models and Hellenistic Diplomacy*, *Journal of Ancient Judaism Supplements* 24 (Göttingen: Vandenhoeck & Ruprecht, 2018). Pp. 494. Translated by Margaret Rigaud. ISBN 978-3-525-55252-0. \$ 95.83.

The original Hebrew text of the First Book of Maccabees did not survive, perhaps because Jews were not interested in preserving a dynastic history dedicated to promoting a line of rulers that did not even last a century. The fact that, to boot, the dynasty adopted Sadduceeism (a type of Judaism that was to be remembered as heretical) halfway into its history, may also have played a role. That the book survived at all, albeit in Greek, is due to its inclusion in the Septuagint. It is, therefore, somewhat ungrateful to complain that scholars and other readers typically come to 1 Maccabees with the expectation that it should be understood as a biblical book. Nevertheless, it is hardly one—if only because biblical books that focus on rulers are typically critical of them, whether from a prophetic point of view or a Deuteronomistic one.

Given the context in which 1 Maccabees was preserved, it requires a serious effort, and willingness to see things as they are, to realize that the book is

not really at home in Sacred Scripture and should be catalogued, instead, among dynastic histories written to serve the interests of secular rulers. Realizing that is difficult even for readers who notice that the book nowhere mentions God (*Theos*) or the Lord (*Kyrios*), and that, after ch. 4, even “Heaven” is hardly mentioned. The ease with which most readers who note those data, and other similar points (no miracles, apparitions, angels, resurrection, or sin-and-atonement; few prayers after ch. 4; martyrs are ineffectual and function as foils for the heroic rebels; high priests are characterized by military prowess [10:21] and wealth [16:11–12]), turn immediately to explaining them away, one way or another, rather than beginning by accepting them as *prima facie* evidence for a lack of interest in God, can be amazing, and is evidence for the effort that is needed to correct that approach.

Katell Berthelot’s monograph makes a substantial contribution to that effort, by undermining a central element of the presumption that 1 Maccabees is at home in the Bible. That element is the assumption that the Hasmoneans, who rebelled against the Seleucids in the 160s BCE and successfully built up, by conquest, a state of their own, viewed themselves as latter-day Joshuas—and their wars of expansion, correspondingly, as fulfilling biblical notions of a holy war for the Promised Land. Instead of those concomitant notions, to which other books of the Bible lead readers of 1 Maccabees, the present volume offers a thesis that is quite easy to follow, in three logical stages spread over the book’s three Parts.

First, negatively, Berthelot argues that the Hasmoneans, in their wars that expanded the borders of Judaea and eventually brought Palestine as a whole under their control, did not understand themselves as following the biblical model of conquering the Promised Land as Joshua once did. While they did compare themselves now and then to biblical heroes, such as David and Solomon, the particular notion of a Promised Land did not guide them. This argument is made, for the most part, on the basis of detailed study of relevant or potentially relevant parts of the Hasmoneans’ court history, 1 Maccabees, which do not support the notion. Next, still in Part 1, Berthelot contends, positively, especially on the basis of 1 Macc 15:33–35, that the Hasmoneans in fact justified their wars on the basis of a Hellenistic model, which pointed either to ancestral rights to territory or to rights endowed by victory in a just war. Finally, in a two-pronged third move, Berthelot surveys in Part 2 the evidence for Hasmonean history after the period covered by 1 Maccabees (i.e., for the most part, Josephus’s accounts of the Hasmoneans beginning with the end of Simon’s rule), which she terms “The Era of the Conquests,” and in Part 3 some additional Jewish literature—several Qumran texts, the Psalms of Solomon, and rabbinic literature. The basic point of both surveys is that those sources too fail to bolster the notion that the Hasmoneans understood themselves (and/or presented themselves) as latter-day Joshuas conquering the Promised Land. Rather, as Berthelot summarizes, “[we do not find] discourse on the promised land, let alone the Holy Land or fatherland, anymore than on the need to recreate the Davidic kingdom [...] the policy of territorial expansion the Hasmoneans followed from Simon, and especially John Hyrcanus, onwards is described in pragmatic terms” (281). By the latter, Berthelot means, more or less, what

she quotes approvingly from Seth Schwartz: “they expanded because they could” (428). Moreover, since many of the texts surveyed in Part 3 bespeak opposition to the Hasmoneans, and especially to the most successful conquerors among them (John Hyrcanus and Alexander Yannai), they contribute to Berthelot’s basic case in a more general way by suggesting that ancient Jews who (as the authors of those texts) cared about the Bible often opposed the Hasmoneans. That reinforces the basic contention that the Hasmoneans were not concerned to guide their behavior by biblical models, nor even to present themselves as if they were.

In general, this is quite a persuasive book. The negative part is especially convincing for, as noted at the outset, once the question is considered with open eyes it is not difficult to become convinced that it is a mistake to read the Hasmoneans’ dynastic history, 1 Maccabees, as if it were at home in the Bible. True, the tendency to read it that way is all the more natural because the book obviously uses biblical diction and biblical style. Nevertheless, careful reading of 1 Maccabees, giving attention both to what it does not say and to what it does, leads clearly to the conclusion that biblical models in general, and biblical notions of the Promised Land in particular, are not what governed the author’s agenda—nor, presumably, the agenda of the Hasmoneans, whose agent he was. Not infrequently biblical verses are echoed or alluded to in 1 Maccabees, whether meaningfully or as mere decoration, but the Promised Land notion was not one that attracted the author’s interest.

That is especially clear insofar as, as Berthelot shows, 1 Maccabees never mentions the Promised Land, never attempts to define its borders, and never identifies any of the Hasmoneans’ enemies as Canaanites—who, as she emphasizes, are the only ones against whom the Bible mandates war of conquest (Deut 20:16–18). Canaanites are mentioned only once in 1 Maccabees, in a passing identification of a woman whose ethnicity was of no interest (9:37), as Berthelot emphasizes (149–153). Moreover, as Berthelot notes (106–107), 1 Maccabees ignores opportunities to comment on the biblical associations of the various sites and battlefields that it mentions, just as more generally it makes, despite some weak allusions, no real attempt to portray the Hasmoneans’ wars as holy wars à la Deuteronomy 20 nor, especially, as entailing *herem*, which is so prominent in the biblical accounts of Joshua’s campaigns. Note that in 1 Macc 5:5, the only passage in the book in which *herem* might have been mentioned in the original Hebrew (ἀνεθεμάτισεν), the victims are the “Sons of Baian,” who are otherwise unknown. Moreover, note that, even apart from the possibility that they lived in Transjordan, beyond the borders of the Promised Land (Berthelot, 134–135), what is important here is that they are not defined as Canaanites and, on the contrary, a specific reason is given, in the preceding verse, to explain why Judas Maccabaeus attacked them. Indeed, concerning Joshua, Berthelot (102–109) notes quite correctly that, despite scholars’ assumptions and statements to the contrary, there is next to nothing in 1 Maccabees that recalls him. He is mentioned only once, at 2:55, in Mattathias’s deathbed speech. That verse is somewhat enigmatic, but for the present purpose suffice it to note that it makes no reference to conquest and that it mentions Joshua as only one entry in a long list of biblical heroes, with no attempt to draw any

special parallel between him and the Hasmoneans. Contrast 2:54, where Phinehas is characterized as “our father,” in order to remind readers of the Hasmoneans’ claim to the high priesthood.

In this connection, I will refer to a study that appeared too late for Berthelot to relate to it: in a study published in 2017, Elchanan Reiner argued that 1 Maccabees’ account of Jonathan’s pre-battle supplication at 11:71 is modeled on Joshua’s at Josh 7:6. (1) This seems indeed to be likely, for both come in the wake of initial defeat and share the same basic elements: rending of garments, putting dust on the head, and prayer—followed by eventual victory. So, to that minor extent, Berthelot’s account of the failure of the book to allude to Joshua should be corrected. However, that correction entails a much more important observation, which significantly supports Berthelot’s basic thesis: although in the end both Joshua and Jonathan were victorious, God is frequently mentioned in the sequel in Joshua (chs. 7–8), as He who controls everything, but never in that in 1 Maccabees. On the contrary: the latter immediately goes on to say that Jonathan inferred from his victory not that God was on his side but, rather, that “the *kairos*” (moment) was “synergetic” (12:1), which in plain English amounts to Jonathan recognizing that he was having a lucky streak, was “on a roll.” Thus, even here, God is left out of the story—a point that corresponds to Berthelot’s basic case about what the Hasmoneans, as characterized by their official history, were really about. The same is the case for references to *kairos* at 9:10 (Judas’s death) and 15:33–34 as well.

That last-named text is of central importance for Berthelot’s case, and she devotes a whole chapter to it (161–185). That is because Simon’s speech, in which he rejects Antiochus VII’s demand that he give up some of his conquests, is often taken to bespeak the Promised Land notion. However, as Berthelot notes, Simon makes no reference to God or to any divine promise concerning the land. Rather, in rejecting Antiochus’s demand that he relinquish territories he had conquered, Simon insists that they were ancestral lands that, at some *kairos*, had been conquered by others while now, at a more congenial *kairos*, the Judeans had retaken them. Berthelot makes two main points: there is no reference to God or to promise, and the formulation of Simon’s response, which denies that he has “taken control” of others’ land and insists that instead it is his fathers’ (Οὐτε ...κεκρατήκαμεν, ἀλλὰ τῆς κληρονομίας τῶν πατέρων ἡμῶν), parallels that of Antiochus himself at 15:3, which complains that others had taken control of *his* fathers’ kingdom (κατεκράτησαν τῆς βασιλείας τῶν πατέρων ἡμῶν). That means, first of all, that Simon’s response is conditioned by the context in the book, and is not an independent statement of ideology. Moreover, to the extent it bespeaks any larger notion, it is not that of a Promised Land but, rather, that it is legitimate to take control of land that once belonged to one’s fathers (cf. 1 Macc 10:52, 55 and 11:9). As Berthelot shows, on the basis of literary and epigraphic evidence (177–185), that notion was quite current

(1) Elchanan Reiner, “The Battle of Qedesh on the Plain of Ḥatsor: On the Hasmonean Roots of the Galilean Foundational Myth,” in *Talmudic Transgressions: Engaging the Work of Daniel Boyarin* (ed. Charlotte Elisheva Fonrobert et al.; JSJ Supplement 181; Leiden: Brill, 2017), 495–506.

in the Hellenistic world. That is, Simon shares Antiochus's notion about what makes for a legitimate claim to territory, disagreeing with him only with regard to the facts of the particular case: Simon insists that Jerusalem is the patrimony of *his* fathers, not Antiochus'. Indeed, as Berthelot argues, the fact that Simon, according to the next verse (15:35), offered to pay restitution for Jaffa and Gezer means that he recognized they were not part of his ancestral territory. Had Simon (or the author of 1 Maccabees) been thinking in terms of the biblical Promised Land there would have been no room for such a distinction, for there is no way those cities could have been assumed to be outside that biblical vision.

For readers of the present journal it is, perhaps, especially apposite to relate to Berthelot's discussion of Qumran evidence, in Part 3 of her volume. She reviews numerous 4Q texts (175, 339, 161, 339, 379, 390, 448, 471a, 522), along with the *pesharim* on Nahum and Habakkuk and the *Temple Scroll*, and, apart from those texts that are too fragmentary to be interpreted, she concludes that they either support her thesis or do not contradict it. The only exception is 4Q471a, a fragmentary text that apparently portrays the Hasmoneans as claiming they fight God's wars.

Most of Berthelot's discussion of Qumran texts is, one way or another, in dialogue with the late Hanan Eshel's *The Dead Sea Scrolls and the Hasmonean State* (2008); sometimes she follows it, sometimes she goes her own way. Note, especially, her detailed discussion of 4Q448, which concludes (in agreement with some predecessors) that most probably it condemns "King Jonathan." Since she identifies that king as Alexander Yannai, the greatest Hasmonean conqueror, her interpretation of this text, which has religious Bible-readers condemning the king, supports her basic thesis in a general way. So do her evident preference (see below) for seeing Yannai as the "Wicked Priest" of *Pesher Habakkuk* (as opposed to Eshel, who saw him as Jonathan, the brother of Judas Maccabaeus, two generations before Yannai and before the period of major conquests) and her observation that the *Temple Scroll*'s revision of the "Law of the King (Deuteronomy 17//Temple Scroll, cols. 57–58) does not contemplate wars of conquest of the Promised Land.

The latter observation is particularly interesting insofar as it comes along with another one, of methodological import: as Berthelot notes, the *Temple Scroll*'s paraphrase (col. 62) of Deuteronomy 20 does not omit the laws of ("holy") war that are set out there, but neither does it develop them. In contrast, its version of Deuteronomy 17's "Law of the King" does indeed depart seriously, one way or another, from the biblical text. As she apparently argues in a footnote (369, n. 92), following Schiffman (2), it seems reasonable to draw historical conclusions from the deviations from the biblical *Vorlage* in the latter text, while not ascribing much importance to the fact that it failed to skip something in another passage. Rewriting requires more effort than copying, and, therefore, testifies to more interest and commitment.

(2) Lawrence H. Schiffman, *The Courtyards of the House of the Lord: Studies on the Temple Scroll* (ed. Florentino García Martínez; Studies on the Texts of the Desert of Judah 75; Leiden: Brill, 2008), esp. 516–517.

Of course, there are plenty of points one could argue about; detailed volumes invite debate. Thus, for three:

- (a) There is something unsatisfying about locating the discussion of *Pesher Habakkuk's* "wicked priest" in a chapter about Yannai but avoiding the question of his identity (388).
- (b) Although Berthelot accepts, in the end, that it is "probably correct" (353) that the accursed "man of Belial" mentioned in 4Q379 was John Hyrcanus, she raises doubts, along the way, as to whether it takes words of Josh 6:26 the way they are used in the original. Namely, that verse uses "this city" of Jericho and בְּכֹרֹו יִסְדְּנָה וּבְצַעֲרֹו יָצִיב דְּלָתֶיהָ to refer to the death of two sons, but Berthelot, citing others, contemplates the possibility that 4Q379 takes the former to refer to Jerusalem and the latter—to the sons' participation in the rebuilding of Jericho. In my opinion, there is little to say in support of either doubt, and it certainly seems to be overdone to conclude, as Berthelot does (349), that the usual reading, and interpretation about the sons, is in fact "not possible." On the contrary, it is the other reading that is quite difficult to defend. It is based on Newsom's reading of the text, but Newsom adduces no support for her assertion that the *bet* of בְּכֹרֹו...וּבְצַעֲרֹו is a "*bet* of means," (3) and indeed it seems to be extremely doubtful, given the fact that Josh 6:26 is explicitly alluded to at 1 Kings 16:34, which takes it, as Newsom admits, in its usually-assumed sense of "at the cost of." The only consideration that occurs to me, which might justify reading a "*bet* of means," is the simple fact that it is after this phrase that 4Q379 reports that "they" built Jericho, so they were not dead. The latter, however, would be significant only if we assume both that (i) the text is describing events in their chronological order (and so it cannot mean that the sons died before they participated in building the city) and (ii) the plural subject implied by שָׁבוּ וּבָנוּ refers specifically to the sons and not to the "Son of Belial's" men in general. I see no justification for either assumption that is so cogent as to require us to posit a chasm between the biblical text and its use. That is possible, of course, but normally we expect stronger evidence.
- (c) Especially given Berthelot's identification of *Pesher Nahum's* "Lion of Wrath" as Alexander Yannai, it is surprising that she relegates to a footnote (385, n. 152) the similarity between the *pesher's* reference to "hanging alive on a tree" and Josephus's reference (*Ant.* 13.380) to Yannai's execution of his enemies—a point that is usually taken, along with the *pesher's* reference to "Demetrius," to be one of the linchpins of the identification of the "Lion of Wrath" as Yannai. The apparent reason for this sidelining of this point is the fact that, as she notes, some scholars have denied the historicity of the incident. However, of the two she cites, one (Joseph Klausner) wrote before *Pesher Nahum* became available, and the other (Joshua Efron) denied the relevance of the Dead Sea Scrolls to the study

(3) Carol Newsom, "The 'Psalms of Joshua' from Qumran Cave 4," *JJS* 39 (1998):

of the Second Temple period—so it is not clear why their arguments should impress Berthelot. (In the margins of this point, I will note another: in that footnote, Berthelot cites Josephus as referring to execution by “*apotympanismos*,” which she defines as “tying them to a pole or plank.” However, that noun never appears anywhere in Josephus. He uses the verb once, at *Against Apion* 1.148, in a sense that is not clear. (4) The verb that appears with regard to Yannai, at *Ant.* 13.380 [as at the parallel at *War* 1.97] is ἀνασταυρόω, which usually means “crucify” or, perhaps “impale”—meanings that correspond well to the *peshet*’s “hanging alive on a tree,” just as New Testament texts will apply “hanging on a tree” [Deut 21:22–23] to the crucifixion of Jesus [Acts 5:30; 10:39; Gal 3:13]. In referring to Josephus’s report as she does, Berthelot adopts an hypothesis expressed by the late Joseph Méléze Modrzejewski who, in his zeal to deny all precedent for crucifixion by Jews, asserted that Yannai, as a Hellenistic *basileus*, would not have adopted a Roman practice, crucifixion, rather than one for which there is more Hellenistic precedent. (5) Note, however, that, in fact, *apotympanismos*, in the sense posited by Méléze Modrzejewski and followed by Berthelot, practically amounted to a type of crucifixion. (6) In any case, why should we not relate to *Ant.* 13.380 the way we usually relate to Josephus’s very similar report about Antiochus IV at *Ant.* 12.256: Josephus reports that Antiochus crucified Jews [ἀνεσταυροῦντο...ἀνεσταυρωμένων], and we can choose between doubting that [especially since his source, 1 Macc 1, does not mention crucifixion] and believing it? If one wants to doubt Josephus’s report about Yannai, why posit that he should have used another verb? Is it not simpler and wiser just to doubt the truth of his statement, and to suppose that Josephus [and/or his source] wanted to blacken Yannai’s memory just as he blackened Antiochus IV’s? And if the latter suggestion is to be rejected because *Peshet Nahum* refers to something of the same, then we are back where we began: why relegate this episode to a footnote, instead of making it one of the main pillars of the case that the *peshet* is condemning Yannai?)

Such points, however, about this or that detail, are immaterial with regard to Berthelot’s main thesis, and that is the case for most others that I noticed along the way. Here I will mention only one more, which is, potentially, of more general import: Berthelot twice states (79, 217), as do many others, that Josephus, according to his *Vita* (§2), “descended from the Hasmoneans through his mother”—a point that, Berthelot implies, motivated Josephus to treat the Hasmoneans positively. However, apart from the fact that such a family connection would only create an a priori possibility that can add little to the analysis

(4) See Arthur Darby Nock, “Thackeray’s Lexicon to Josephus,” *Harvard Theological Review* 25 (1932): 361–362.

(5) Joseph Méléze Modrzejewski, “Alexandre Jannée et les Pharisiens,” *Zeszyty Prawnicze* 15 (2015): 19. He simply declares that “il est tout à fait invraisemblable” that Yannai crucified his enemies; rather, since he styled himself a Greek *basileus*, we should assume he used a Greek mode of execution.

(6) Indeed Liddell-Scott-Jones renders the verb as “crucify on a plank.”

of what Josephus actually wrote about the Hasmoneans, note that, in fact, what he says about his personal connection to the dynasty refers to something much weaker than a mother-child relationship. Namely, after using the abovementioned general formulation (ἀπὸ τῆς μητρὸς) in §2, in §4 he explains that he meant that, well over a century before his birth, one of his male ancestors married a daughter of a Hasmonean high priest (Jonathan). (7) This significantly attenuates Josephus's personal connection to the dynasty and, therefore, the cogency of this *a priori* expectation about how to read his narratives about the dynasty.

A more general issue, however, should also be raised. Throughout the book, Berthelot asks what motivated the Hasmoneans in their campaigns that expanded the borders of their kingdom, and concludes that it was not the notion of a Promised Land or the desire to emulate Joshua. Rather, she argues, what motivated them was the realization that they *could* build a state at the Seleucids' expense, and that they were *justified* in doing so because the territories in question were ancestral patrimony. That all seems reasonable to the present reviewer. What is not clear, however, is why the Hasmoneans would forego the opportunity to claim to be fulfilling divine promise as well; what smart politicians pass up such options? Indeed, it seems clear, from such passages as 1 Macc 5:62 and 14:4–15, as well as from John Hyrcanus's claim to have been a prophet (*Ant.* 13.282, 299), that they did claim divine support when that was useful for them. Why, then, would they not suggest to their subjects that their successes, in conquering the land, were a fulfillment of the ancient mandate to do so—a mandate once fulfilled by Joshua and now by themselves? Why, in other words, did they not do what modern politicians might do, that which was done with 1 Maccabees in the consensual interpretation with which Berthelot takes issue? It would be interesting to hear her thoughts about that.

For my part, I would suggest first that perhaps they did it sometimes (as seems to be echoed by 4Q471a), but that, insofar as usually they did not, we have here an aspect of a basic division within of the Hasmonean dynasty, one that Berthelot too employs. Berthelot distinguishes between the early ones, down to Simon, who fought a war of liberation, and the later ones, beginning with John, who fought wars of expansion, and it is this distinction that leads her to prefer to date Qumran criticism of the Hasmoneans to the days of the latter; the more they conquered, the more they were criticized. Whatever one says about that (and what it entails about, for example, the identification of the Wicked Priest), I would suggest that those two halves of the Hasmonean dynasty differed in another way as well: the earlier Hasmoneans were more deferential to Jewish tradition. That is well reflected both in the prayers and references to God that survived in the first four chapters of 1 Maccabees, which presumably reflect early materials, and also in Simon's willingness, in 140 BCE, both to forgo the royal title and to qualify his line's high-priesthood as one that would be valid only so long as no "faithful prophet" showed up and made new

(7) See esp. Max Radin, "The Pedigree of Josephus," *Classical Philology* 24 (1929): 193–194.

arrangements (14:41). Concessions such as those avoided provoking subjects who maintained hopes for a renewed Davidic monarchy and who—perhaps still remembering the Oniads—were not comfortable, or worse, with the rise of a new family to the high priesthood. The same restrained attitude could have brought the early Hasmoneans to avoid presenting themselves, pretentiously, as Joshua redivivi; and anyway those early Hasmoneans did not conquer much. The later Hasmoneans, in contrast, with their successes, their riches, their mercenaries, and their Hellenistic concepts, had no need to claim biblical support, or God's, for their conquests. Indeed, the religious opposition that they provoked, reflected in Qumran texts and in Josephus's reports of rebellion (*War* 1.67, 88–98; *Ant.* 13.299a, 372–383), might well have prevented them from claiming such, even had they wanted to.

The volume's clear division into chapters and numerous subdivisions, along with a fifteen-page index of ancient sources cited in the book, make it very easy to follow the argument and otherwise navigate the book. Moreover, although a translation, the book is also quite readable; the inevitable faux amis (such as “lessons” for “readings”), and some other gallicisms, here and there, add some charm to the book, and in fact testify, by their rareness, to the translator's general success. There has been, in recent years, a burgeoning of scholarship on the Hasmoneans, but Berthelot's volume is a very welcome and useful addition.

Daniel R. SCHWARTZ

Lindsey A. Askin, *Scribal Culture in Ben Sira*, Supplements to the Journal for the Study of Judaism 184 (Leiden / Boston: Brill, 2018). Pp. X + 311. ISBN 978-90-04-37285-6. € 115.00 / US \$ 138.00.

In the revised version of her doctoral dissertation written under James K. Aitken's supervision at the University of Cambridge, Lindsey Askin investigates “how Ben Sira wrote his book of wisdom” (p. 1). She thereby focuses on both creative operations like compositional techniques and the “physical handling of texts” (p. 1)—what Askin calls “scribal culture” (p. 1). She argues that “Ben Sira's textual reuse becomes a conscious performance of textual dexterity and strategy, not simply creativity or originality” (p. 1).

After a review of scholarship on Ben Sira (pp. 4–15) and some methodological remarks (pp. 15–20), Askin illustrates Ben Sira's location in “scribal culture” and then offers five case studies on Ben Sira's scribal method.

Askin develops the term “scribal culture” in two different ways. First, she argues that the physicality of reading and writing in the ancient world must be taken into consideration far more than is usually done in current scholarship. By referring to ancient literature and iconographic material, she (convincingly) points out that the ancient scribe must not be imagined as working on a desk and handling several books. He instead sat on the floor with the parchment or papyrus on his thighs. She concludes that the method of both copying and composing literature is either to read a passage first and then to write it down from memory or to have a text dictated. In a second section, Askin highlights

the lack of material finds for an educational system in Judah and Jerusalem and how one can deal with this lacuna.

This first chapter proves crucial for Askin's study and therefore deserves attention. From a methodological point of view, it should be noted that Askin refers solely to secondary literature and investigations by others. This is tolerable as this chapter is not her main topic in the book; nevertheless, it would have been appropriate to work with primary material. More important is a methodological question. Her description switches between epochs and places without discussing the problems of this approach. Material remains are scant, and they are spread all over the Near East. However, it is important to ascertain the basis of one's conclusion, especially if they form the fundamental background of the investigation.

Nevertheless, the consequences for the methodology in biblical exegesis are important and should be considered more broadly. For this very reason it is a pity that when returning to her foundation throughout her study, Askin never goes further than the quite general observations and remarks in her opening chapter. Furthermore, the question of the process of translation, which is vital in Ben Sira research, is not even touched.

The second chapter of her book serves as an example in this regard. Askin first investigates Ben Sira's reuse of the Noah passages from Genesis in Sir 44:17–18. She is quite reliant on the results of source criticism in the Genesis flood story and finds the Priestly wording reflected in Ben Sira's reception of Gen 6–9. Unfortunately, she does not correlate this with other passages in the book of Ben Sira and especially with the fact that Ben Sira's wording is quite inconsistent. Furthermore, she admittedly ascertains the differences of Ben Sira's use of scripture(s) to other texts' use of the same account, but she does not sufficiently reflect the different genres of writing and these genres' impact on the process of writing, composing, and rewriting texts. Finally (and after a second case study on Phineas), she concludes with a confirmation of her argument in the first chapter (pp. 69–76).

In chapter 3, Askin presents two examples of Ben Sira's use of long sections of biblical texts with multiple versions. With regard to Ben Sira's method of text appropriation, she points out that it proves an intellectual handling based on reading and then composing one's own text thereafter. However, she explicitly includes the assumption that Ben Sira constantly consulted his primary sources during his writing process. Also of interest is that by comparing the different versions and especially in the Hebrew textual transmission, Askin demonstrates that Ben Sira used a copy or copies of the biblical texts that are closer to the Masoretic text than to Qumran.

In chapter 4, Askin works on the references to those passages in Job and in Psalms related to weather phenomena (Job 36–41; Ps 104; 147; 148). She thereby shows the plausibility of Ben Sira's use of a Psalm scroll with a similar order to the Qumran Psalms scrolls 11QPs^a and 4QPs^d.

Chapter 5 investigates the references to Job and Qoheleth that focus on death, and chapter 6 addresses Ben Sira's perspective on medicine and its background. In both cases, Askin is able to confirm her model of scribal culture while including extra-biblical and non-Jewish writings mainly from the Hellenistic period.

The author closes her study with some conclusions that summarize her results. A bibliography and indices of references to source texts are provided in the appendix.

Askin's study on the scribal culture in Ben Sira is an interesting and rewarding work on the book of Ben Sira with regard to both its intellectual heritage and historical environment and its compositional methods on display through reference to other texts.

Frank UEBERSCHAER

Carmen Palmer, *Converts in the Dead Sea Scrolls: The Gēr and Mutable Ethnicity*, Studies on the Texts of the Desert of Judah 126 (Leiden: Brill, 2018). Pp. xii + 231. ISBN 978-90-04-37817-9. € 105.00 / US \$ 126.00.

In this study, Carmen Palmer seeks to answer the question: "what is the meaning of *gēr* when the term is used in the Dead Sea Scrolls (DSS)?" (1). Does *gēr* already designate a "Gentile convert to Judaism," as it does in rabbinic literature? And, what place did the *gēr* have in the sectarian movement associated with the DSS? In brief, Palmer argues, "the *gēr* employed in texts that draw on the technique of scriptural rewriting in the DSS is a convert, although variably included or excluded within the movement. A *gēr*-convert is a prior Gentile who converted to Judaism and is assumed to have undergone circumcision, and joined the movement affiliated with the Damascus tradition. However, this same Gentile convert to Judaism, represented in the *gēr*, is found to be excluded from the movement affiliated with the Serekh tradition" (4–5). In support of this conclusion, Palmer provides a thorough analysis of all instances of *gēr* in the DSS (chapters 2–4) before turning to a "sociohistorical comparison" of the use of the term *gēr* in the DSS and similar ideas in Greco-Roman associations (chapter 5).

Chapter two begins with an overview of the sectarian movement associated with the DSS, a movement comprised of at least two contemporary "streams" reflected in the Damascus (D) and Serekh (S) traditions. Next, Palmer establishes the text, date, and provenance of thirteen instances of the term *gēr* in the DSS and then categorizes the texts based on their relationship to D and S (a text that influences D and S, texts correlated with D, texts correlated with S, and texts correlated with the sectarian movement but whose alignment with D or S is indeterminate).

In chapter three, Palmer analyzes these instances of *gēr* by "look[ing] for scriptural rewriting, that is, the recognizable reuse of scripture, that will highlight changes made to a text or idea over time" (93). As a result, she argues that these texts use *gēr* to refer to a Judean convert who has had a real change in kinship and connection to the land. Additionally, Palmer suggests: (a) "the D tradition accepts Gentile converts to Judaism as members, and considers Judean ethnicity to be mutable and open to Gentiles," and (b) "[i]n the S tradition...it appears that Judean ethnicity is immutable and closed to Gentiles" (127).

Chapter four seeks an explanation for “how and why levels of ethnic mutability differ between D and S traditions” (130). To this end, Palmer focuses on three features of the ethnic identity of the *gēr* in the DSS: shared kinship, connection to land, and circumcision. With regard to the use of kinship language to refer to the *gēr*, especially references to the *gēr* as a “brother,” Palmer argues that “a fellow ‘brother’ in the D tradition of the sectarian movement is Judean, while a fellow ‘brother’ in the S tradition of the sectarian movement is ‘supra-Judean’” (137). Moreover, while circumcision is seen as an important feature of ethnic identity and conversion in the DSS, the S movement “add[ed] a secondary, metaphorical and spiritual circumcision, in addition to an initial physical circumcision” (152). Therefore, Palmer concludes that “within the S tradition, members of S are in fact ‘supra-Judeans’ who undergo an extra ‘circumcision of the heart.’ This ‘supra-Judean’ nature of members helps to explain why the S tradition does not accept regular Gentile converts to Judaism as does the D tradition” (130).

Finally, chapter five offers a sociohistorical comparison of the use of brother language in the DSS and Greco-Roman associations. Such a comparison, Palmer suggests, could further corroborate the conclusions drawn from the preceding literary analysis. She draws attention to the fact that “a newfound and constructed shared kinship between previously noncosanguinal ‘brothers’ is evident in cultic associations where multiple features of ethnic identity are dominant” (163). Different from the DSS, “the [cultic] association’s adopted brothers are parallel to cosanguinal siblings, through the ideal of Greek and Roman adoption” (182).

Palmer’s study offers a commendably comprehensive and judicious analysis of the meaning of *gēr* in the DSS and contributes significant insights to our understanding of the communities reflected in the DSS, particularly related to conversion, membership, and conceptions of ethnicity. Consequently, she draws our attention to some of the most fundamental distinctions between the communities associated with the D and S traditions. Finally, her final chapter demonstrates the benefits of comparing ancient Jewish communities to Greek and Roman associations, especially cultic associations, an approach that has the potential to yield many other insights in the study of ancient Judaism and Christianity.

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